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THE PSYCHOLOGICAL REVIEW

BASIC NEURAL MECHANISMS IN BEHAVIOR *

BY K. S. LASHLEY

Behavior Research Fund, Chicago

Among the systems and points of view which comprise our efforts to formulate a science of psychology, the proposition upon which there seems to be most nearly a general agreement is that the final explanation of behavior or of mental processes is to be sought in the physiological activity of the body and, in particular, in the properties of the nervous system. The tendency to seek all causal relations of behavior in brain processes is characteristic of the recent development of psychology in America. Most of our text-books begin with an exposition of the structure of the brain and imply that this lays a foundation for a later understanding of behavior. It is rare that a discussion of any psychological problem avoids some reference to the neural substratum, and the development of elaborate neurological theories to 'explain' the phenomena in every field of psychology is becoming increasingly fashionable.

In reading this literature I have been impressed chiefly by its futility. The chapter on the nervous system seems to provide an excuse for pictures in an otherwise dry and monotonous text. That it has any other function is not clear; there may be cursory references to it in later chapters on instinct and habit, but where the problems of psychology become complex and interesting, the nervous system is

* Address of the President of the American Psychological Association before the Ninth International Congress of Psychology at New Haven, September 4, 1929.

dispensed with. In more technical treatises the neurological explanations are made up mostly of assumptions concerning the properties of the nerve cell which have no counterpart in physiological experiment. Thus we find the superiority of distributed over concentrated practice seriously 'explained' by the 'fact' that successive passage of neural impulses over a synapse reduces its resistance least when the impulses come in quick succession.

There is no direct evidence for any function of the anatomical synapse: there is no evidence that synapses vary in resistance, or that, if they do, the resistance is altered by the passage of the nerve impulse. If the explanation is to be given in terms of established facts, as it must be, then it is limited to the following form: the superiority of distributed practice is due to the discontinuity of the neurons, the polarity of conduction, the fact of learning, and the superiority of distributed practice.

This is a typical case of the neurological explanations to be found in our psychological literature. With such conditions prevailing, it seems time to examine critically the relations between psychology and neurology and to attempt an evaluation of current notions concerning the mechanisms of the brain.

INADEQUACY OF CURRENT THEORIES

The starting point for our attempts to account for behavior in terms of nervous processes has been either the cerebral localization of functions or the theory that all nervous integration is patterned after the spinal reflex. I need scarcely point out the difficulties encountered by the older doctrine of cerebral localization. It expresses the fact that destruction of definite areas results in definite symptoms and the probable inference that these different parts have diverse functions, but it has given us no insight into the manner in which the areas or centers exercise their functions or the way in which they influence one another. It is only by applying psychological conceptions like that of association, or by turning to the theory of reflexes that the doctrine of localization is made to express the dynamic relations of behavior.

The extension of the theory of reflex conduction, first derived from studies of the spinal cord, to problems of cerebral function provided a welcome addition to the psychophysical doctrine of localization. It gave a clear interpretation of localized areas as relay points or centers along the course of the reflex arc and seemed to explain the functional relations of the areas. However, the theory has not worked well in application to the details of behavior. To understand the difficulties we should have clearly in mind the form and limitations of the theory. It states that the mechanism of cerebral function is essentially the same as that of the spinal reflexes, involving the conduction of nerve impulses from the sense organs over definite, restricted paths to the effectors. The performance of a habit, whether of speech or of manipulative movement, is determined by the existence of definite connections between a limited number of nerve cells, which are always functional in that habit. The model for the theory is a telephone system. Just as two instruments can be connected only by certain wires, so the sense organs and muscles concerned in any act are connected by nerve fibers specialized for that act.

Perhaps few neurologists would agree to such a bare statement. They point to the incalculable number of nerve cells, the interplay of inhibition and facilitation, and suggest that in so complex a system there are limitless possibilities. But the fact remains that the essential feature of the reflex theory is the assumption that individual neurons are specialized for particular functions. The explanatory value of the theory rests upon this point alone, and no amount of hypothetical elaboration of connections alters the basic assumption.

Both the doctrines of localization and of conditioned reflexes imply the correspondence of structural and functional units—the specialization of minute areas or of single cells for definite limited functions. Recent experimental and clinical evidence seems to show that there is no such correspondence, and thus to present fatal difficulties to both theories. I shall sketch the main lines of this evidence, then turn to a consideration of other possible mechanisms.

Analysis of the Adequate Stimulus

The notion of the reflex arc was developed in studies of spinal preparations in which protopathic stimuli or muscle tensions are the chief sources of excitation. Under these simple conditions something like a point for point correspondence between receptor cells and muscle groups could be demonstrated, as in the case of the scratch reflex.

We first attempted the extension of this conception to instinctive behavior, on the assumption that the adequate stimulus to nursing, to the recognition of the mate or young, to the recognition of the nest site, to sexual excitement might be expressed in terms of the excitation of such and such receptor cells. This proved to be a vain hope. The adequate stimulus in such cases may be described in terms of a pattern having definite proportions but always, within wide limits, it is a matter of indifference to what receptor cells this pattern is applied.

A survey of various types of behavior shows that this is an almost universal attribute of the adequate stimuli.[1] It is most obvious in pattern vision and can be demonstrated in animals with a rather primitive cortex. I have recently improved the technique for study of vision in the rat so that habits of pattern vision may be established in 20 or 30 trials. It is thus easy to test the equivalence of stimuli under conditions where previous associations are ruled out. Not only do we find transposition as Köhler has described it for chimpanzees, but even more striking equivalencies. An animal trained to discriminate patterns of solid white on a black ground is undisturbed by reversal of the brightness relations, by substitution of outlines for the solid figures, or even by partial outlines which retain some of the proportions of the original figures.

In many cases it is clear that the equivalent stimuli involve none of the retinal elements which were activated during learning. Here we have a situation where a habit is formed by the activation of one set of receptors and executed im-

[1] K. Goldstein, Die Topik der Grosshirnrinde in ihrer klinischen Bedeutung, *Dtsch. Zsch. f. Nervenheilk.*, 1923, 77, 7–124.

mediately upon stimulation of an entirely different and unpracticed group. The equivalence of stimuli is not due to the excitation of common nervous elements. The equivalent patterns have in common only ratios of intensity or of proportion in the spacial distribution of excited points. I might multiply examples of this sort indefinitely, but the studies of the Gestalt psychologists leave little doubt that such a condition is the rule for all stimuli with which we deal in the study of behavior.

Analysis of Reactions

Turning to motor activity, we are confronted by an identical problem. If we train an animal in a maze and observe carefully his subsequent errorless running, we find little identity of movement in successive trials. He gallops through in one trial, in another shuffles along, sniffing at the cover of the box. If we injure his cerebellum, he may roll through the maze. He follows the correct path with every variety of twist and posture, so that we cannot identify a single movement as characteristic of the habit.[2]

I have earlier reported cases of the direct adaptive use in the performance of motor habits of limbs which were paralyzed throughout training and whose motor paths consequently could not have been exercised during training.[3] It is not helpful to say that previously formed general habits are utilized in such performances, for the preexisting habits have not been associated with the new situation and the problem of the spontaneous association of the new patterns remains unsolved.

The problem of equivalence of motor responses has been less studied than that of equivalence of stimuli, but the phenomenon seems to be equally common. Activities ranging from the building of characteristic nests by birds to the so-called purposive activities of man show the absence of stereotyped movements in the attainment of a predetermined

[2] K. S. Lashley and D. A. McCarthy, The survival of the maze habit after cerebellar injuries, *J. Comp. Psychol.*, 1926, 6, 423–433.

[3] K. S. Lashley, The theory that synaptic resistance is reduced by the passage of the nerve impulse, Psychol. Rev., 1924, 31, 369–375.

goal. The most familiar and most striking example is that of grammatical form in speech. Once we learn a new word, we use it in correct grammatical relations in limitless combinations with other words, without having to form new associations for each new setting.

It is only in certain acts of skill that stereotyped movements are recognizable and the uniformity of these is a result of long practice. We seem forced to conclude that the same motor elements are not necessarily used in the learning and performance of motor habits and that motor elements can be utilized directly when no specific associations have been formed with them.

Plasticity in Central Organization

Studies of the central nervous system give a similar picture. The functions are relatively independent of the structural elements. I can only cite a few of the lines of evidence, but sufficient, I believe, to establish the point.

First with respect to the specificity of conduction paths. The final motor neurons have been studied by Weiss.[4] He grafted additional limbs on salamanders, cutting the nerve which supplied the original limb so that the regenerating fibers came to innervate both the original and the new limb. The two limbs innervated by the same nerve showed synchronization of movements in corresponding muscle groups. Histological examination showed that the axons of the original nerve had branched so that the muscles of the two limbs were supplied by fibers from the same axons. There is no selective outgrowth of regenerating fibers and the branches of the same axon do not necessarily go to corresponding muscles. It seems, then, that the coördination of the two limbs is not a function of the particular fibers which innervate each muscle, but is due to some property of the nerve impulse such that the same fiber can selectively elicit either of two antagonistic movements. These experiments are still the subject of controversy, but the objections raised against the results are not particularly impressive and, though they may raise some

[4] P. Weiss, Die Funktion transplantierter Amphibienextremitäten. *Arch. f. mik. Anat.*, 1924, 52, 645–672.

doubt of this conclusion, they certainly do not establish the specificity of the axon. The results of Weiss are in harmony with many facts revealed by the study of the central nervous system.

In work with injuries to the spinal cord Miss Ball and I [5] have found that orientation of the rat in the maze is undisturbed by interruption in the cervical cord of either the pyramidal, rubrospinal, or any other of the long descending tracts. The impulses controlling turning and threading the maze somehow get down the cord after the destruction of any half of the descending fibers. I have more recently been working with double hemisections of the cord. In these preparations one half of the cord is divided in the upper cervical region, the other half below the nucleus of the phrenic nerve, so that all the long fibers are interrupted above the motor centers for the limbs. After three months such preparations show coördinated movements in walking and are able to control the limbs for orientation in response to stimuli applied to the head. The control is established in spite of the permanent interruption of all the long spinal paths.

We have also been accumulating evidence upon the functions of the projection and association tracts of the cerebrum in the rat. The data are not yet complete, but it seems fairly certain that the interruption of the projection fibers to a part of a functional area produces far less pronounced symptoms than destruction of the cortical area supplied by those fibers. We have now a large number of cases in which linear lesions sever the connections between the different anatomical areas of the cortex or divide the association fibers within single areas. It is rare that any symptoms can be detected in such cases, unless there is involved a considerable destruction of cortical tissue. The most capable animal that I have studied was one in which the cortex and underlying association fibers had been divided throughout the length of each hemisphere. His I.Q., based on ten tests, was 309.

In higher forms there is evidence for a somewhat greater

[5] K. S. Lashley and J. Ball, Spinal conduction and kinæsthetic sensitivity in the maze habit, *J. Comp. Psychol.*, 1929, 9, 70–106.

specificity of long tracts in the central nervous system, but even in man the evidence is unequivocal only for the pyramidal system, which we have reason to believe is a part of the postural system and not especially concerned in the higher integrative functions of the brain, and for sensory paths of the cord. Although I would not venture the opinion that the association tracts of the cerebrum are a skeletal structure, there is certainly no direct evidence for the existence in them of any sharply defined reflex paths whose interruption results in the loss of isolated elementary functions.

What is the evidence that the cortex itself contains the definite specialized synapses which are demanded by the reflex theory? The data from extirpation experiments are somewhat ambiguous, but taken as a whole, fairly conclusive. Small lesions either produce no symptoms or very transient ones, so that it is clear that the mechanisms for habits are not closely grouped within small areas. When larger areas are involved, there are usually amnesias for many activities. Some of our experiments show that the degree of amnesia is proportional to the extent of injury and, within wide limits, independent of the location of the injury. This may mean that the cells differentiated for the habits are widely and uniformly scattered, or that there are no especially differentiated cells. After injuries to the brain, the rate of formation of some habits is directly proportional to the extent of injury and independent of the position within any part of the cortex. This shows that the rate of learning is not dependent upon the properties of individual cells, but is somehow a function of the total mass of tissue. Rate of change in individual synapses does not express the facts of learning unless we postulate some means by which the capacity for change in any cell is modified by the activity of all other cells of the cortex. Finally, when such habits have been formed after brain injury, their retention correlates with the amount of functional tissue. This can be interpreted only as evidence that memory is not a function of individual cells, but is a property of the total mass of tissue.[6]

[6] K. S. Lashley, Brain mechanisms and intelligence, Chicago, Univ. Press, 1929, pp. 186.

The reflex theory is not helpful for an understanding of such facts, nor do they seem consistent with it. If we consider the whole reaction, from sense-organ to effector, the impossibility of a theory of specialized intercellular connections becomes apparent. Let us analyze a visual reaction, for here the anatomical localization of paths seems best established. The observations of Marie and Chatelain,[7] and of Holmes and Lister [8] suggest a detailed projection of the retina upon the cortex, the macula represented in the posterior calcarine region, and successive radial zones along the borders of the fissure. (I am not sure that this interpretation is correct. Poppelreuter [9] has pointed out that the forms of scotoma are not as varied as the manifold shapes of lesion should lead us to expect, and that all the forms of scotoma can be interpreted as radiating or converging disturbances of the functional balance within the entire area. I have observations of a migraine scotoma in which the blind area retained a characteristic shape but drifted from the macula to the periphery of the visual field in the course of half an hour.) But granting a cortical retina, the problem of integration is only moved back a step. I have cited evidence to show that the retinal cells used in the formation of a habit need not be excited in order to reinstate the habitual response. This must be equally true, then, for the cortical retina. The same cells may not be twice called upon to perform the same function. They may be in a fixed anatomical relation to the retina, but the functional organization plays over them just as the pattern of letters plays over the bank of lamps in an electric sign.

We find then at the point of projection on the cortex a variable pattern shifting over a fixed anatomical substratum. How can this elicit a response from a definite set of motor cells? It can not do so by excitation over definite association paths, for there is evidence against the existence of such paths and, besides, there are no fixed points of origin for them.

[7] P. Marie, et C. Chatelin, Les troubles visuels dus aux lésions des voies optiques intracérébrales et de la sphère visuelle corticale dans les blessures du crâne par coup de feu, *Rev. neurol.*, 1914–15, **28**, 882–925.

[8] G. Holmes, and W. T. Lister, Disturbances of vision from cerebral lesions with special reference to the macula, *Brain*, 1916, **39**, 34–73.

[9] W. Poppelreuter, Die psychischen Schädungen durch Lopfschuss, Leipzig, 1917.

Nor is it certain that there are any fixed motor points. We have found in studies of the motor cortex that a point which will elicit a primary movement of the fingers on one day may, a week later, produce a movement of the shoulder and at another time even movements of the face.[10] And the motor cortex, with its somewhat definite localization, is probably not concerned in habitual activity, anyway.

There does not seem to be a possibility of a constant anatomical localization at any point from receptor surface to effectors. Somehow the motor system must be sensitized to respond to the sensory patterns, but the phenomena cannot be expressed in terms of definite anatomical connections. This is the fundamental problem of neural integration and must serve as the starting point for any adequate theory of cerebral function.

The Doctrine of Circular Reflexes

An essential element of the reflex theory as applied to psychological problems is the doctrine that all the effects of stimulation are immediately observable in the motor systems. The James-Lange theory of emotion, the idea that mental attitudes are an expression of bodily postures or 'sets,' the theory that instincts and serial habits are chains of sensory-motor activity, the doctrine that implicit speech or gesture forms the basis of thinking: these are all expressions of the belief that the nervous system serves merely for the rapid switching and conduction of impulses from receptor to effector, without long-continued intraneural sequences of activity. This notion has been attractive, as offering a possibility of direct objective study of mental activity, but attempts to verify it experimentally have given disappointingly negative results.

The problem of emotion is still in such confusion that one can draw no conclusions with confidence, but the accumulation of evidence upon the variability of expressive reactions and the repeated failure to find any consistent correlations between bodily changes and either exciting situations or

[10] K. S. Lashley, Temporal variation in the function of the gyrus precentralis in primates, *Amer. J. Physiol.*, 1923, **65**, 585–602.

reported subjective states lends little support to the visceral theory.

On the question of maintained attitude or set we have some recent evidence which seems significant. Studying the influence of bodily posture upon the movements elicted by stimulation of the motor cortex Dr. Jacobsen and I mapped the motor area and selected for study a point giving extension of the fingers. We changed the posture of the limbs, head, and body of the preparation, stimulated muscles and nerve points electrically and in other ways sought to alter the conditions of peripheral stimulation. The excitability of the point was unaltered by this treatment and the same movement was elicited at five-minute intervals for two hours. We then altered the excitability of the point by stimulation of another distant point, changing the primary movement from extension to flexion. This new primary movement persisted for 55 minutes in spite of repeated changes in the posture of the animal, then reverted spontaneously to the original movement of extension. The experiment suggests that the pattern of organization of the motor cortex can be altered by central excitation and that the altered condition can be maintained for long periods without reinforcement from peripheral organs. It seems to fulfill the conditions for demonstration of a centrally maintained attitude.

Miss Ball and I have tested the effects on serial habits of sectioning the afferent paths of the cord, together with removal of all external directive clues after the animal is oriented in the starting box. Under these conditions the habits are run off without disturbance. With external and internal sensory cues eliminated it seems that the series of acts must be controlled by some wholly central mechanism.

The work of Thorson on tongue-movements [11] and unpublished observations on eye-movements during thinking, together with reports of the recovery of speech with use of an artificial larynx, oppose the doctrine of the completed reflex and point to some continued intraneural process as the basis of thinking. The weight of evidence, I believe, favors the

[11] A. M. Thorson, The relation of tongue movements to internal speech, J. Exper. Psychol., 1925, 8, 1–32.

view that in emotion, in all persistence of attitudes, in all serial activity there are continuously maintained central processes which, if they become intense, may irradiate to motor centers and produce expressive movements, implicit speech, and the like. The pattern of irradiation varies from subject to subject according to chance variations in the excitability of the motor or vegetative nervous systems, and the peripheral activities are not an essential condition for the maintenance of the central processes.

I have devoted so much time to criticism of the reflex theory of behavior because it seems to be deeply rooted in our thinking and to have had an important influence in the development of almost every phase of psychology. It has been valuable in counteracting certain trends toward vitalism and mysticism, but I believe that it is now becoming an obstacle rather than a help to progress. In the youth of a science there is virtue in simplifying the problems so that some sort of decisive experiments may be formulated, but there is a danger that oversimplification will later blind us to important problems. In the study of cerebral functions we seem to have reached a point where the reflex theory is no longer profitable either for the formulation of problems or for an understanding of the phenomena of integration. And if it is not serviceable here, it can scarcely be of greater value for an understanding of the phenomena of behavior.

THE ALTERNATIVE TO THE REFLEX THEORY

What is the alternative to the doctrine of the specialization of nervous elements for definite reactions? It is possible that the modes of organization in the brain are not less numerous and diverse than the types of behavior to which they give rise. We have little direct evidence as to the nature of these central processes, but can deduce some laws from the effects of cerebral injury which may point the way to the significant investigations of the future.

Dynamic Aspects of Localization

Specialization of functions in the cerebral cortex is an indisputable fact, but we have yet to find an adequate inter-

pretation of it. We have asked, Where are psychological functions localized in the brain? and have gained a meaningless answer. We should ask, How do specialized areas produce the details of behavior with which they are associated: what are the functional relationships between the different parts and how are they maintained?

Variable Degrees of Localization.—If we survey the disturbances produced by brain injuries in a wide range of activities we are forced to the conclusion that the accuracy of localization or the degree of specialization varies greatly. Definitely limited defects appear in the visual and tactile and to a lesser extent in the motor fields after limited lesions to the calcarine, postcentral and precentral gyri. In other sensory spheres and in all the more elaborate organizations of behavior, there is little evidence for an equal fineness of differentiation. The visual cortex probably represents the maximum of specialization of small units. In the somesthetic field there is also a cortical projection, but less finely differentiated. In other functions we find every degree of specialization up to the limit where all parts of the cortex participate equally in the same function. The latter is apparently the condition for the maze habit in the rat. Destruction of any part of the cortex produces a partial loss of the habit and equal amounts of destruction produce equal amounts of loss, regardless of locus within the cortex.

An area which is highly specialized for one function may play a more generalized rôle in another. The habit of brightness discrimination in the rat is abolished by injury to the area striata, and by injury to no other part of the cortex. Here is a clear case of specialization. But the maze habit is abolished by destruction of this same area or of any other of equal size. Is it because the maze habit is dependent on vision? No, for blinding trained animals does not affect the habit, whereas destruction of the area striata abolishes the habit in animals which were blind during training. The deterioration does not differ in any observable way from that following lesions to other parts of the brain.

Except in projection areas there is no evidence for ana-

tomical specialization within the general areas of localization. Thus in the aphasias showing predominantly a loss of naming ability or of memory for words there is not a selective effect upon memories for specific words, but a general difficulty of recall which embraces all words of a functional group.

The evidence on localization suggests that where the relations of stimuli in space are of importance for behavior, there exists in the cortex a spacial distribution of points corresponding to the sensory surfaces, but that for all other functions a similar spacial arrangement is lacking. In terms of the reflex theory such a spacial arrangement has little meaning, but in terms of the hypothesis to which I am leading it is of prime importance.

Functional Levels of Organization.—Turning to the dynamics of localization, we find that loss or partial loss of functions may find expression in various ways. In some cases it seems that the fundamental organization for a function has been very little disturbed but that the ease of arousal is markedly altered. Thus in monkeys and probably in man, the severity of cerebral paralysis varies somewhat with the current emotional state, and during great excitement the power of voluntary movement may be temporarily restored. The paralysis seems to consist of a greater or lesser difficulty in initiating movements, whose organization is undisturbed.[12] The emotional facilitation can restore the capacity for movement. It clearly does not supply the specific integrations but only makes the final common paths more excitable or increases the intensity of activity in the integrating mechanisms. Here we have the energy for activity supplied, as it were, from an outside source. Some of the symptoms of cerebellar ataxia and the conditions described as pure motor aphasia present the same sort of picture. I have used the term energy here with reluctance, for the notion of nervous energy has led to many extravagant speculations, yet it seems impossible to deal with such phenomena except in terms of some general factor which may influence the ease of functioning of a system of activities without changing the specific integrations.

[12] M. Minkowski, Etude physiologique des circonvolutions rolandique et pariétal, *Arch. Suisse de neurol. et psychiat.*, 1917, 1, 389–459.

In another type of quantitative reduction in efficiency, the integrative mechanism itself seems affected, but without disintegration into elementary functions. In the rat, destruction of the occipital cortex abolishes the habit of brightness discrimination in the Yerkes box. Brightness vision is actually undisturbed, as can be demonstrated by other methods, but the association with the specific activities of the training box is disturbed. The amount of practice necessary to reestablish the association is closely proportional to the extent of lesion. Here we are dealing with some function akin to the memory trace of Ebbinghaus. Just as the memory trace grows weaker with the passage of time, so it is weakened by cerebral injury. Recall may be impossible, yet a persisting trace of the former training may be demonstrated by the "savings method." The strength of the trace is determined by the quantity of tissue. The efficiency of performance is determined by the summated action of all parts of the area.

We cannot here use the accepted theories of summation or reinforcement, for these theories are based upon the phase relations of nerve impulses and we seem to be dealing with a continuous summation. It seems impossible to express the facts in other terms than simple variation in energy.

The Relative Fragility of Functions.—I have pointed out that the same area may be involved in quite diverse functions. These may be differently affected by lesions. Thus the habit of threading a complex maze is seriously disturbed by destruction of any part of the cortex, provided the lesion involves more than 15 per cent. The habit of a simpler maze is unaffected by lesions involving as much as 50 per cent of the cortex. We do not have an extensive series of tests with different mazes, but a comparison of Cameron's cases [13] with my own indicates that there is a definite relationship between the complexity of the maze habit and the minimal lesion which will produce a measurable disturbance of it.

Dr. Jacobsen has similar evidence from experiments with monkeys.[13a] Animals were trained to open a series of simple

[13] N. Cameron, Cerebral destruction in its relation to maze learning, PSYCHOL. MONOG., 1928, 39, (No. 1), 1–68.

[13a] To be reported soon in *J. Comp. Neur.*

puzzle boxes and also a box in which the latches of the simple boxes were combined. After destruction of the frontal or parietal lobes, the ability to open the simple boxes was retained, but the same latches in combination could not be opened.

We have similar results on the limits of training for both the rat and monkey. Simple problems may be learned at almost normal rate after brain injuries; complex problems are learned slowly, if at all. Further, the greater the brain injury, the greater is the disproportion between the learning of simple and complex habits. In such cases the brain injuries seem to limit the complexity of organization which may be acquired, without disturbing the capacity for the simple acts which are mediated by the same areas.

The clinical literature presents many comparable cases. The aphasic patient may be able to understand and execute simple commands and yet be unable to grasp the same instructions when several are given at the same time. Head cites numerous instances of this limitation in complexity of organization.[14] In pattern vision, the stages through which the patient passes during recovery from cortical blindness form a series with respect to complexity. It seems probable that the great fragility of color vision and of the perception of depth is due to the high degree of organization required for these functions, rather than to their separate localization in the cortex.

Both the animal experiments and the clinical material point to the conclusion that a given area may function at different levels of complexity, and lesions may limit the complex functions without disturbing the simpler ones. Further, we cannot ascribe this limitation to the loss of some necessary elementary functions or to disturbances of nutrition or to shock, for it has been shown in some cases to be solely a function of the quantity of tissue. In this respect the limitation of complexity seems to accord with Spearman's view [15] that intelligence is a function of some undifferentiated nervous energy.

[14] H. Head, Aphasia and kindred disorders of speech, New York, 1926.

[15] C. Spearman, The abilities of man, New York, 1927.

The Relational Framework in Cerebral Function

Let us turn now to another important aspect of cerebral function. A review of symptoms suggests that no logically derived element of behavior can be shown to have a definite localization; no single sensation, memory, or skilled movement is destroyed alone by any lesion. On the contrary the various parts of the functional areas seem equipotential for such elements, and either a whole constellation of them is affected by the lesion, or none at all. In these constellations of activities the grouping is determined, not by associative bonds, but by similarities of organization. Let me illustrate this point. In an hysterical amnesia we may find a loss of memory for all events associated with some emotional experience. The constellation is here determined by the grouping of habits. In organic amnesias the grouping is quite different. The speech most commonly retained in aphasia is that related to emotional expression, as was pointed out by Hughlings Jackson. Ejaculations, words of affirmation or negation, profanity, and words having a deep personal significance make up the residual vocabulary. The determining factor here seems to be the relation to emotional facilitation. Head's work shows other groupings, the loss of words involving the object-name relation, or of those concerned with the relations of space, time, and logical order.[16] The defects can in every case be related to ways of thinking about things, but not to loss of specific associations.

Many disturbances of vision show the same characteristics. An apparent word-blindness, for example, may be due, not to a loss of visual memory for the words, but to an inability to see the letters in a definite spacial arrangement.

Even in experiments with animals there are suggestions of similar conditions. We find rather frequently a picture which suggests loss of the general sense of direction, with retention of associations with the specific turns in the maze.

I have not time to multiply examples, but I believe that there is ample evidence to show that the units of cerebral function are not single reactions, or conditioned reflexes as

[16] H. Head, *op. cit.*

we have used the term in America, but are modes of organization. The cortex seems to provide a sort of generalized framework to which single reactions conform spontaneously, as the words fall into the grammatical form of a language.

The Mutual Influence of Cerebral Activities

Every statement concerning independence of functions must be made with certain reservations. The early students of aphasia, using the crudest of examining methods, were able to identify a great variety of types involving entirely different defects in the use of language. More careful study of the aphasias with finer methods of examination has failed to reveal such clear-cut distinctions, and today we have no accepted classification of the aphasias. Marie concluded that all types are the result of a common intellectual defect. Head denies that the intellectual defect is primary but admits that in all cases intellect suffers to the extent that it employs symbolism in thinking. All investigators seem now agreed that the disturbance of speech is only one symptom of a disorder which can be traced through the whole fabric of thought.

This is typical of recent developments in the clinical field. With improvement in methods of examination, the complete isolation of functions becomes more and more questionable, until it seems as though disturbance in any function implies lesser, but recognizable changes in every other.

This interdependence is not merely an expression of the subtraction of elementary functions by brain injuries. It seems to involve a genuine fusion of different processes, such as is shown in the observations of Poppelreuter [17] on the 'totalizing function' of the visual area, and those of Gelb on the mutual influence of normal and hemiamblyopic areas. Poppelreuter reports the completion of simple figures in the cortically blind field, much like the normal filling in of the blind spot. Gelb [18] describes a case in which objects were

[17] W. Poppelreuter, *op. cit.*

[18] A. Gelb. u. K. Goldstein, Psychologische Analysen hirnpathologischer Fälle, Leipzig, 1920.

judged smaller when seen in one half of the visual field than when seen in the other. When exposed so as to include both fields they were judged intermediate in size. Evidently in such cases as this there is fusion rather than summation of elements.

Self Regulation in Neural Function

This unity of action seems to be more deeply rooted than even the structural organization. In working with animals and with human patients I have been more and more impressed by the absence of the chaotic behavior which we might expect from the extent and irregular form of the lesions. There may be great losses of sensory or of motor capacities, amnesias, emotional deterioriation, dementia—but the residual behavior is still carried out in an orderly fashion. It may be grotesque, a caricature of normal behavior, but it is not unorganized. There are certain apparent exceptions to this rule, such as the loss of control of laughter in certain thalamic and lenticular cases, jargon in aphasia, the loss of spacial organization in some cases of visual agnosia, but even here the disturbances are not chaotic. Even dementia is not wholly unintelligent. It involves reduction in the range of comprehension, in the complexity of the relations which may be perceived, but what falls within the patient's range is still dealt with in an orderly and intelligible fashion.

There seems always to be a certain spontaneous compensation or adaptive reorganization. The most definite example of this sort is the observations of Fuchs [19] on pseudofovea. He finds that in cases of complete hemianopsia there is a shift of the center of fixation from the anatomical fovea to a variable point in the peripheral retina which acquires a greater visual acuity than can be demonstrated in the anatomical macula.

Such phenomena suggest that the nervous system is capable of a self-regulation which gives a coherent logical character to its functioning, no matter how its anatomical constituents may be disturbed. If we could slice off the cerebral cortex, turn it about, and replace it hind side before,

[19] W. Fuchs, Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker. In Gelb und Goldstein, *op. cit.*

getting a random connection of the severed fibers, what would be the consequences for behavior? From current theories we could predict only chaos. From the point of view which I am suggesting we might expect to find very little disturbance of behavior. Our subject might have to be reeducated, perhaps not even this, for we do not know the locus or character of habit organization—but in the course of his reeducation he might well show a normal capacity for apprehending relationships and for the rational manipulation of his world of experience.

This may sound like a plunge into mysticism, but an example from another field will show that such self-regulation is a normal property of living things. Wilson and later Child [20] have crushed the tissues of sponges and hydroids, sifted the cells through sieves of bolting cloth and observed their later behavior. The cells are at first suspended independently in the water, but may be brought into aggregates by settling or centrifuging. Starting as flat sheets, they round up into spherical masses and begin differentiation. Embryonic stages may be simulated and eventually adult individuals with characteristic structures, mouth, hypostome, tentacles, and stalk in normal relative positions are produced. In spite of the abnormal conditions to which it is subjected, the formless mass of cells assumes the structure characteristic of the species. Of course many abnormal forms appear, but even these follow the characteristic scheme of organization.

Many lines of evidence show a close parallelism between the facts of morphogenesis and those of the organization of the nervous system. In both we have given as the fundamental fact an organization which is relatively independent of the particular units of structure and dependent upon the relationships among the parts. In both there is a capacity for spontaneous readjustment after injury, so that the main lines of organization are restored; in both there is evidence that every part may influence every other; in both there is a possibility of dissociation and independent activity of some parts.

[20] C. M. Child, Axial development in aggregates of dissociated cells from Cory morpha palma, *Physiol. Zool.*, 1928, 1, 419–461.

The Mechanisms of Organization

This brings us to the question of the mechanisms by which organization of behavior is brought about. There is, I think, nothing mysterious about the problem. There is no need to assume an emergence of new properties, a transcendent influence of the whole upon the parts, a subordination of substance to form, or the like; there is certainly no need to look for nonphysical agencies. We are dealing with a complex system in which there is an influence of every part upon every other, with all degrees of intimacy in the relations and various degrees of dominance and subordination. Our problem is to discover the means by which these influences are exerted.

We have seen that the notion of isolated reflex paths, exerting mutual inhibition and facilitation and conducting nervous impulses over pathways determined by the specific resistance of synapses, is not only inadequate to account for the simplest facts of behavior, but is also opposed by direct neurological evidence. The greatest progress in neurophysiology within the past decade has been made in the study of conduction in peripheral nerves, but the results have as yet little bearing upon the problems of central organization. At most they offer a basis for speculation concerning the behavior of nerve impulses at intercellular junctions, and recent negative results upon conduction with decrement throw some doubt upon the value of these speculations. Students of nerve conduction have taken for granted the doctrine of anatomical specialization, and their work has not been developed, in the direction of our problems. Lapicque [21] has recently pointed out some of the difficulties of the anatomical hypothesis and has suggested the substitution of temporal for spacial factors in organization, but the study of chronaxie is not far enough advanced for application to the problems of psychology. The laws of conduction in nerve fibers thus far revealed are not alone sufficient for an understanding of integration. The nervous unit of organization in behavior is not the reflex arc, but the mechanism, whatever be its nature, by which a reac-

[21] L. Lapicque, The chronaxic switching in the nervous system, *Science*, 1929, 70, 151–154.

tion to a ratio of excitations is brought about. We have as yet no direct evidence upon this problem, but the similarities of the problems of nervous function and of growth should direct our interest toward the processes which have been found important in the control of structural development.

The work of many students of experimental embryology has shown the importance of the restriction of gaseous interchange, of gradients in chemical diffusion, metabolic activity or rate of growth, the influences of chemical and electrical polarization and of the flow of action currents in determining the course of development. During its first differentiation the nervous system is subject to the same influences as any other developing tissue and the mechanisms of diffusion and of polarization play an important rôle in the determination of its structures and inherent organization. It would be strange if, with the completion of growth, these factors should no longer be important in the life of the cells. Rather, we should expect the neurons to be continuously modified by the same influences. The structure of the nervous system is such as to allow of this. The interconnections of distant parts are well insulated, where correlated functions without influence of intermediate parts is required, but within the gray matter the cell bodies and processes are not so protected. They are directly exposed in a liquid medium capable of conducting diffuse chemical and electrical changes which may readily influence the excitability of the neurons. The arrangement of the gray matter in thin sheets and the projection of the receptor and motor surfaces upon these sheets may have a real functional significance. Child [22] has shown that distance of separation favors the development of independently polarized systems, and the arrangement of cell bodies in the gray matter offers the optimal condition for this and for the development of systems in which the spacial arrangement of stresses can be effective. Although the distant intercommunications of cells may be solely through the conduction of nerve impulses, the more immediate coördinations within the gray matter may depend upon relative amounts of excitation, the spacial

[22] C. M. Child, Studies on the axial gradients in Corymorpha palma. iii, *Biologia Generalis*, 1926, 2, 771–798.

arrangement of excited points, stress patterns resulting from the total mass of excitation, which may be more important for behavior than the connections of individual cells. It is here, I believe, that we must look for the next significant development in our knowledge of the functions of the brain.

Cerebral organization can be described only in terms of relative masses and spacial arrangements of gross parts, of equilibrium among the parts, of direction and steepness of gradients, and of the sensitization of final common paths to patterns of excitation. And the organization must be conceived as a sort of relational framework into which all sorts of specific reactions may fit spontaneously, as the cells of the polyp fit into the general scheme of development.

Such notions are speculative and vague, but we seem to have no choice but to be vague or to be wrong, and I believe that a confession of ignorance is more hopeful for progress than a false assumption of knowledge.

CONCLUSION

I have devoted my time to-night to problems which are not strictly psychological, yet I believe that these problems are of real significance for the progress of psychology. Certainly the development of the science up to the present has been strongly influenced by neurological theory. The frantic search for sources of motivation and of emotion in visceral activity, though initiated by introspective analysis, has been supported by the faith that the nervous system is only a conductor having no sources of energy within itself. Our preoccupation with analysis of learning by trial and error, the denial of association by similarity, the belief that transfer of training can occur only through the training of common synapses—these are a result of the belief that learning is simply a linking together of elementary reflexes. The doctrine that the intelligent solution of problems results only through random activity and selection, and that intelligence itself is an algebraic sum of multitudinous capacities, is largely a deduction from the reflex theory.

I shall not pretend to evaluate such doctrines from the

standpoint of psychological evidence. They may or may not be true, but their truth must be demonstrated by experiment and cannot be assumed on a background of questionable neurology. Psychology is today a more fundamental science than neurophysiology. By this I mean that the latter offers few principles from which we may predict or define the normal organization of behavior, whereas the study of psychological processes furnishes a mass of factual material to which the laws of nervous action in behavior must conform.

The facts of both psychology and neurology show a degree of plasticity, of organization, and of adaptation in behavior which is far beyond any present possibility of explanation. For immediate progress it is not very important that we should have a correct theory of brain activity, but it is essential that we shall not be handicapped by a false one.

The value of theories in science today depends chiefly upon their adequacy as a classification of unsolved problems, or rather as a grouping of phenomena which present similar problems. Behaviorism has offered one such classification, emphasizing the similarity of psychological and biological problems. Gestalt psychology has stressed a different aspect and reached a different grouping; purposive psychology still another. The facts of cerebral physiology are so varied, so diverse, as to suggest that for some of them each theory is true, for all of them every theory is false.

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THEORY OF CONDITIONING AND OF RELATED PHENOMENA

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A close examination of about 200 original reports of experiments on conditioning and on negative adaptation has led to the following generalizations:

1. When two or more organismic reaction-patterns are repeatedly brought into action, either simultaneously or in close succession, the repertory of the organism's reactions will, as a rule, increase in magnitude, in number, or in both, the stimulus of the less dominant [1] pattern becoming finally attached to the response of the more dominant pattern and *thereby* detached from its own response.

Let *s–O–r* be the less dominant pattern, withdrawing from a faradic shock, and *S–O–R* the more dominant pattern, subsequent salivation to powdered biscuit in the mouth; then at first the change observed is *S–O–**R***, or an increase in the magnitude of the more dominant pattern, while later a new pattern, *s–O–R*, appears, *s–O–r* becoming subdued.[2] The initial stage, usually known as facilitation, has been in a few instances the only indication of modifiability obtained by the experimenter (Cason's lid reflex, Yerkes' experiment on the frog, Kreps' on Ascidiacea).

2. When only one isolated pattern is repeatedly activated,

[1] Dominance in conditioning seems to depend upon specific dominance of certain patterns, succession in time, intensity of stimulus, present physiological state, and past conditioning history of each pattern involved. It will be maintained here that all these factors are specific instances of intensity of reaction, although such a contention can be supported—for the lack of measurements in comparable units of the intensities of various reactions—only indirectly. The writer regrets that space does not permit him to discuss fully the evidence for this article; a detailed discussion will be found in a forthcoming publication by the writer of a statistical and critical analysis of all experimental work in conditioning.

[2] Nociceptive reactions do not show in conditioning that degree of dominance which they assumedly exhibit at the spinal level. (M. Erofeeva, Electrical stimulation of the skin of the dog as a conditioned salivary stimulus, Thesis, St. Petersburg, 1912.).

the organism's repertory of reactions will decrease in magnitude, in number, or in both.

(*a*) Let *S–O–R* or *s–O–R*, an unconditioned or a conditioned pattern respectively, be repeatedly exercised, then response–*R* will be gradually reduced and finally disappear. The disappearance of pattern *s–O–R*, known as extinction, is usually faster than that of *S–O–R*, known as adaptation, mainly because, when the magnitude of pattern *s–O–R* is decreased by repetition, it loses its dominance to the old pattern *s–O–r*.

(*b*) Let *S–O–R*, a compound pattern in which one stimulus brings about a number of responses, be repeatedly exercised, then gradually more and more of the less dominant parts of the pattern, $S–O–r_1$, $S–O–r_2$ . . . $S–O–r_n$, will weaken and drop out leaving only the most dominant part *S–O–R*. This has usually been known as 'strengthening by use' but it is also largely operative in the kind of modifiability known as 'selection by trial and error.'

It is the second generalization which is the more universal, while the first has to be qualified by the following:

(a) Two or more patterns may be activated together, with the result that they will all be reduced or completely disappear when they are combined, but separately the stimulus of each pattern will still elicit its original response without a decrement. This will occur when the degrees of dominance of the patterns approach each other. It has been mainly studied, under the name of 'conditioned inhibition,' when one of the patterns was already conditioned, but it has been also observed in repeated combinations of two previously unconditioned patterns, as in associating a faradic shock to the skin with an acid in the mouth.[3]

(b) Numerous combined activations of two or more patterns may result in their fusion and consequent behavior

[3] If one of the patterns is less dominant, as mechanical stimuation, or the other more dominant, as food in the mouth, conditioning is successful (Erofeeva, p. 130). Similarly, an increase of the interval between the applications of the two stimuli in conditioned inhibition, a step equivalent to the weakening of the first pattern, results in the conditioning of the stimulus of the first to the response of the second pattern, thus forming a C-R of the second order (D. S. Fursikov, The relation between the processes of excitation and inhibition, *Transac. Pavlov's Physiol. Lab.*, 1925, 1, 1–47).

as one pattern. It is then necessary to add to them a new pattern in order to prevent the decrease or even the disappearance of the response upon repetition (Shishlo, Petrova, Podkopaev, and others).

3. Any modification of organismic behavior will be weakened by disuse, the weakening being negatively accelerated. When tried after a lapse of time, an organismic pattern will therefore show an increase, a decrease, will disappear, or reappear, depending mainly upon the kind, amount, and recency of its last course of modifications, but also, in varying smaller degrees, upon all its preceding modifications.

The first two generalizations as well as the qualifications are well accounted for by a Theory of Dominance.

THE THEORY OF DOMINANCE: *When two or more units, or groups of units, of organismic action are activated simultaneously or in close succession, the action in the more dominant units, or groups of units, will thereby become heightened, while that in the less dominant will thereby become lowered.*

In one form or another, such as 'dominance,' 'drainage,' 'interdependent irradiation,' the existence of some such sort of interdependence and dominance at various levels of organismic action has been assumed and differing accounts for it put forth by Avenarius, James, McDougall, Von Uexküll, Horsley, Meyer, Child, Ukhtomski, Beritoff, and others. But, while behavior data from psychology, physiology, and psychopathology have again and again forced such a hypothesis to the forefront, it has fallen in disrepute with many on account of its supposed conflict with the commonly accepted theory of the nature of the nerve impulse and the 'all-or-nothing' principle (Bayliss, Howell, Lashley, Dodge, and others).

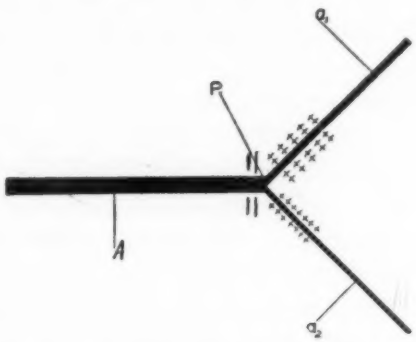
It will, however, be contended here, furthering the start made by Beritoff, that not only is the acceptance of such organismic interdependence and dominance plainly necessitated by all known facts of conditioning and of related phenomena, but that, if non-essential hydraulic analogies are dropped, it is compatible with, and even strengthened by modern knowledge of nerve action. Such an hypotheses

would be at variance with modern neurophysiology only if the functional states of all neural units were largely alike, and if it was then forced to assume a quantum theory of nerve energy and suppose, as Bayliss thought that it must, that "when a nerve fiber is divided into two, the magnitude of the propagated disturbance is half in each of the branches." But it neither need assume the latter, nor is the former by any means true. The investigations of Erlanger, Lapicque, Cattell, Mathews, and others clearly show that nerve fibers differ widely as to threshold, refractory phase, chronaxie, rate of conduction, ability to cross and recoverability from a region of decrement. Chronaxie and rate of conduction have been definitely proven to depend upon the diameters of the nerve fibers, a dependence which should equally hold for collaterals and terminal arborizations, and probably even for dendritic branchings. It is also, of course, further true that complete neural paths may further differ from one another in functional readiness, both because of an unequal number and kind of neurones in each and because of unequal innervation from other sources,[4] as well as possibly on account of interneuronic differences.

Interdependence and dominance may thus be based upon differences in nerve action of different neural units or groups of units the dominance of which will, at any one time, depend both on their characteristic and their present functional readiness and intensity of reaction. To use the now common likening of nerve conduction to explosions in trains of gunpowder, an analogy usually held against organismic interdependence, let us suppose the following: Interconnected but insulated trains of gunpowder, differing in cross-section, in degree of dryness, in quality and size of individual grains, having to cross places of varying dampness, are exploded at different points and designed to charge various forts. Would not the speed of explosion in each train, as well as the magnitude of the charge on each fort, depend respectively not only upon its own explosive state, or states of the trains

[4] It is now well established that the greater the intensity of the stimulus the more frequent is the discharge of each fiber (E. D. Adrian, *J. Physiol.*, 1929, 67, 119–151).

leading to the particular fort, but also upon that of all other trains, so that the explosion in the functionally better trains will be the faster the slower it is in the functionally inferior trains, and conversely? Similarly, let a nerve fiber be divided into two branches of unequal diameters and consequently of unequal rates of conduction; or, let two or more neurones, or groups of neurones, of unequal functional readiness emerge from a common adjustor or center,—would not the amount of the propagation of the nerve disturbance from the common point to each branch of fiber, neurone, or group of neurones, vary not only directly with its own functional state but also inversely with the state of every other unit,



or group of units, of its field of action? A diagram may serve as an illustration:

Let the wave of negativity along the main fiber '*A*' reach '*P*,' the point of its bifurcation into 'a_1' whose diameter and admittedly whose degree of polarization is greater than that of 'a_2,' then the magnitude of the local bioelectric circuit of, and at continual innervation conduction along, either branch will vary not only directly with its own degree of polarization and P. D. with '*A*,' but also inversely with those of the other branch. With the undoubted diverse ramifications, interconnections, and wide differences in the functional states of various units of the nervous system, such a process of interdependence and dominance of the more active over the less active units may evidently be taken as the rule.

Evidence for Dominance: The evidence for dominance from

sources other than conditioning, such as from studies in dynamogenesis, facilitation,[5] binocular rivalry and contrast, distraction, and physiological gradients is well known and will not be cited here specifically. The grasping reflex of the frog which is reinforced by any other stimulation (Albertoni, Baglioni, Ufland) and Ukhtomski's experiments on swallowing and defecation may be perhaps mentioned as the most definite instances. In the latter case, various points of the cortical motor areas of cats and dogs were electrically stimulated during the act of swallowing or defecation and it was found that the usual response to the direct electrical stimulus, such as the movement of a limb or of any other organ, was absent, while the swallowing or defecation was definitely reinforced.[6]

The evidence from the broader field of conditioning is, however, just as convincing. If an electrical stimulus to the skin and food in the mouth are combined to form a conditioned salivary response, it is observed that the more the current is conditioned, or the greater the salivation that it produces, the smaller is the magnitude of its original response. That this is not due to mere adaptation is proved by a control experiment in which the shock was applied under otherwise similar conditions but without the food. Still more clear-cut does the case for dominance become when the behavior of *C–R*'s in their transition from a generalized to a differentiated state is closely examined. As is well known, in the initial stage of the formation of a *C–R* there are established not only the new main pattern *s–O–R* but also weaker connections between similar stimuli and similar responses, or diagrammatically,

Later, however, when *s–O–R* is repeatedly applied with rein-

[5] The refractory phase theory of facilitation and inhibition is very unsatisfactory. See Adrian (*J. Physiol.*, 1929, 67, p. 145) who has himself abandoned it.

[6] A. Ukhtomski, The dependence of cortical motor reactions upon adjacent central effects, *Transact. St. Petersburg Soc. Natur. Sci.*, 1911, 41, Ser. 2, 179–392; German abstract, 393–412).

forcement, the weaker patterns drop out (applications of the weaker patterns without reinforcement are essential only for finer discriminations). As this disappearance of the weaker patterns is not due to disuse, for disuse, on the contrary, makes the *C–R* more generalized—perhaps because disuse reduces the differences between the degrees of dominance of the weaker and the stronger [7] patterns—the only assumption left is that the mere exercise of a stronger pattern causes the weakening and finally the disappearance of weaker patterns.[8] These two instances from conditioning have been brought forward as exhibiting dominance in a most undoubted form. That dominance offers a complete and uniform account of all facts of conditioning and of related phenomena without recourse to additional hypotheses will be shown in the following pages.

Dominance as Accounting for the Fundamentals of Conditioning

The Formation of a C–R.—Any biological theory of conditioning must first assume that there is some sort of connection, let it be subminimal, between any stimulus and any response, and that the differences among the various connections are only a matter of differences in some sort of thresholds, an assumption well authenticated by the fact that, when some area is poisoned by strychnine, resulting apparently in the lowering of some thresholds, its effectors may be brought into action by any stimulus. It must further suppose that the activation together of two patterns produces some *specific* lowering of the threshold [9] between the stimulus of

[7] Weaker and stronger are used in a general sense for less and more dominant.

[8] Dodge's experimental test of drainage cannot be cited against dominance. It is by no means certain that voluntary innervation is dominant over reflex innervation; the reverse should be expected, and consequently the change, if any, would be the reinforcement of the reflex, provided the voluntary preceded the reflex innervation. In point of fact, however, the voluntary followed the reflex response, and the possibility of any change would be exceedingly remote. Of course, as McDougall pointed out, negative results could, in general, not be conclusive here.

[9] The terms 'threshold' and 'lowering of threshold' are used here in a most general behavior sense, as neurophysiology has supplied us so far with little conclusive evidence of their nature. Neurobiotaxis, an increase in neurofibrils, changes in chronaxies, in permeability of membranes, in concentration ratios of various ions have been pro-

one and the response of the other pattern, since in conditioning, unlike strychnine poisoning, it is not *any* but a more or less specific stimulus that comes to elicit the conditioned response. The Theory of Dominance further advances a dominance relationship between the conditioning and the conditioned pattern, basing itself largely, but by no means solely, upon the fact that with the same quantity of food in the mouth as the unconditioned stimulus both very weak and very strong auditory or electrical stimuli are much less conditioned than similar conditioned stimuli of medium strengths.

The formation of a *C–R* is then conceived as follows: When a certain stimulus, let us say an electrical shock, is applied to the skin, its excitation is propagated mainly to its own dominant adjustors and effectors, but also in various subminimal—without overt response—degrees to all other centers, the food center included. When now the food pattern is activated, the preceding subminimal irradiation from the shock stimulus to the food response is, by the new excitation, strengthened, and more and more so by repetition, until its path becomes dominant over the original path of the shock stimulus, the application of which will henceforth—before further modifications—be propagated predominantly to the food effector. The further observation that it is always the stimulus of the less dominant, or weaker pattern, that becomes attached to the response of the more dominant, or stronger pattern may be accounted for by the fact that a subminimal irradiation from a stimulus of a weaker pattern to a response of a stronger pattern, *s–O–R*, will, by an additional excitation of a stronger pattern, become strengthened to a degree of becoming dominant over its original pattern, *s–O–r*, sooner than a subminimal irradiation of a stronger pattern, *S–O–r*, will, by additional excitation of a weaker pattern, become dominant over its original pattern, *S–O–R*.

pounded as the mechanisms of the lowering of the threshold. Of these, a change in permeability, due to different ionic concentrations, is perhaps the most suggestive, as both stimulation and an increase of certain ions, usually monovalent, do seem to increase permeability, while an increase of other ions, usually polyvalent, decreases permeability (Höber, Lasareff, Ebbecke, Mitchel, and others).

This may be illustrated mathematically: Let x be the adequate excitation of the original path of a weaker pattern s-O-r, Kx the adequate excitation of the original path of the stronger pattern S-O-R, $\frac{x}{a}$ the subminimal irradiation of the weaker pattern s-O-R, $\frac{Kx}{a}$ the subminimal irradiation of the stronger pattern S-O-r, $\frac{x}{b}$ the additional strengthening by the weaker pattern, $\frac{Kx}{b}$ the additional strengthening by the stronger pattern,—then, for s-O-R to become dominant over s-O-r, either $\frac{x}{a} + \frac{Kx}{b}$ or $\frac{Kx^2}{ab}$ must be greater than x, when the magnitude of the additional strengthening is assumed to be respectively completely independent of, or totally proportionally to that of the preceding subminimal irradiation; while, for S-O-r to become dominant over S-O-R, $\frac{Kx}{a} + \frac{x}{b}$ or $\frac{Kx^2}{ab}$ must be respectively greater than Kx. It will be seen that, K being greater than unity, the inequalities for the dominance of s-O-R over s-O-r will always be sooner satisfied than those for the dominance of S-O-r over S-O-R, no matter what the relationship between the magnitudes of the subminimal irradiation and those additional strengthening be, for the former will be satisfied when $\frac{K}{b} > \frac{a-1}{a}$ or $\frac{Kx}{b} > 1$, while the latter will be satisfied only when $\frac{1}{b} > \frac{K(a-1)}{a}$ or $\frac{x}{ab} > 1$, and, of course, $\frac{K}{b} > \frac{1}{b}$, $\frac{K(a-1)}{a} > \frac{a-1}{a}$, and $\frac{Kx}{ab} > \frac{x}{ab}$. It will be noted also that, for conditioning to take place, K, or the ratio of the dominance of the conditioning to that of conditioned pattern, must be greater than $\frac{ab-b}{a}$ or $\frac{ab}{x}$, when the relations of the magnitudes of the additional strengthening to those of the preceding subminimal irradiation are respectively of complete independence or of complete dependence.

As to the locus of the modifiabilities of the paths, it would seem from a consideration of the relationships of *C–R*'s to one another as well as from the experiments of Lashley and of Lang and Olmstead that it lies more in the field of action of the conditioning than in that of the conditioned pattern and probably very little, if any, in the peripheral path of the

latter. The greater modification of the conditioning path is particularly strongly suggested by the great amount of transfer [10] of formation or of extinction in homogeneous *C–R*'s, or *C–R*'s of the same conditioning pattern, and the comparatively little transfer in heterogeneous *C–R*'s even of the same conditioned pattern.

Extinction and Negative Adaptation. Theory of Asynchronization.—The reduction and final disappearance of a *C–R* upon its repeated non-reinforced applications has been both the crux of aligning its data with those of other forms of learning and the pivot of all theories of conditioning. Fatigue, internal inhibition, backward association, and biological incongruity have been propounded, but their inadequacies, which are discussed elsewhere,[11] have already been pointed out largely by previous writers. A new Theory of Asynchronization by Dominance is therefore offered. As a preliminary, however, to this theory, the similarity of extinction to negative adaptation is first stressed. The two have been kept separate in the past partly, perhaps, because of a general tendency to dichotomize behavior into native and acquired, but largely, probably, on account of the prevalent view that extinction is always much more rapid than adaptation, and that it exhibits, in addition, some special behavior phenomena. A closer examination, particularly of the more recent experiments, reveals, however, that the ease of extinction, while usual in certain patterns and under certain conditions, is by no means characteristic of all conditioning, and that, furthermore, adaptation shows the same specific behavior as extinction (Beritoff, Liddell, Humphrey, and others). The impairment and final disappearance of a pattern upon repetition is then taken as the most general fundamental phenomenon of

[10] Transfer is used here when, after one C-R is formed or extinguished, another will be formed or extinguished with greater ease, but will not be present or absent at the first trial; by another term, generalization, is meant either that a stimulus similar to an active conditioned stimulus will, without additional training, elicit the same C-R, although usually in a smaller degree, or that a C-R will become extinguished without additional training upon the extinction of some other C-R. The distinction between the two terms is of significance and its confusion by previous writers is to be avoided.

[11] In a forthcoming publication by the writer.

behavior modifiability, in spite of the large amount of contrary evidence from maze running, poem reciting, problem solving, and the like, for the mere reason that impairment by repetition has been observed in simple reaction-patterns, both native and acquired (salivary, patellar, flexion, lid, postural, vestibular, and psychogalvanic reactions as well as the so-called tropisms of lower animals), and under well controlled conditions, while improvement by repetition has manifested itself in much more complex acts, in which both the activation of more than one pattern and the entrance of more factors than simple repetition are highly probable.

The Theory of Asynchronization, in itself, may be stated as follows: *The repeated isolated activation of a neural path will cause the nerve disturbance to be propagated more and more into the more dominant, or functionally superior, and less and less into the less dominant, or functionally inferior, nerve fibers and their branchings, resulting in a widely asynchronous delivery of impulses, or even in a complete discontinuance of impulses from the less dominant units, and consequent weakening and final disappearance of the total response.*[12] This Theory of Asynchronization, in addition to its being in line with the general theory of dominance and with the well established fact of asynchronous delivery of impulses in reflexes, gains support from a number of sources. Thus, Erlanger has proved that the decrement of an action wave during propagation is due to differences in the rates of conduction of individual axons [13]; Adrian has shown that the weaker the reflex response the wider the asynchronization [14]; while Beritoff and others have revealed the existence of subminimal excitation in extinction, as particularly seen, when three *C–R*'s had been completely extinguished, the simultaneous application of their stimuli

[12] The usually assumed insulation of nerve fibers from one another is not an objection to the theory, as, first, there is still possible an asynchronization of collaterals and terminal arborizations, and, secondly, each main fiber is in functional contact with the stimulus whence the unequal distribution of the disturbance would proceed While stated in terms of neural action, the theory means to cover also other organismic action.

[13] Erlanger, Bishop, and Gasser, Analysis of the action potential wave in nerve, *Amer. J. Physiol.*, 1926, 78, 531–573.

[14] E. D. Adrian, *J. Physiol.*, 1929, 67, 119–151.

elicited a response, although none was produced when each stimulus had been given separately.[15] The theory is also greatly strengthened by elimination of other accounts of decrement by repetition in terms of existing knowledge of nerve action. Two other alternatives are possible: first, that repetition produces some change by which the frequency of each fiber is so increased that its impulses fall within its refractory phase and thus become subminimal; second, that some other change is produced by which the frequency of each fiber becomes so decreased that the discharge is again subminimal. The first alternative loses by the fact that, according to it, a stronger reaction—and consequently, as Adrian has shown, a reaction of greater frequency of discharge—should become sooner worn out than a weaker reaction, a fact which is contrary to all experimental evidence. The second hypothesis has to assume that exercise fundamentally diminishes activity in each unit, an assumption which, besides being generally unsatisfactory, is in opposition with some results that stimulation does increase permeability, decreases chronaxie, and thus improves conduction in each unit. The Theory of Asynchronization, on the other hand, while admitting that stimulation in general increases nerve action, still accounts for the impairment of a neural path by repetition, and is not open to either of the preceding objections. The theory can, finally, be tested out experimentally, although it must be said that previous experiments on the effect of repetition on the action current cannot be brought to bear upon it, as, first, the intervals used between successive repetitions were by far too small and thus introduced factors other than those under consideration here, and, secondly, there was no technique of registering individual axon currents.

Why Is There No Asynchronization in Conditioning?—An objection may be raised that asynchronization, if effective in the repetition of one pattern, should also cause a decrement when more than one are combined and thus decrease a reinforced *C–R* by repetition. In the long run, if the combined repetitions are very numerous, it often does, as it was stated

[15] J. Beritoff, *J. f. Psychol. u. Neurol.*, 1927, 33, 116–335.

before. Ordinarily, however, the decrement by asynchronization may be conceived to be outbalanced by the increment from the action of the original path of the conditioned stimulus, since two stimuli and two patterns operate to produce one response. In general, it may be further supposed that where the number of fibers involved in the pattern is large and their frequencies are high, a process offsetting the effects of asynchronization and levelling the conductivity of the fibers may set in, as the conductivity of the best fibers may become so improved and their frequencies so raised that their impulses fall within their refractory phases and their dominances are thus lost. The slight decrement which is usually observed in the repetitive stimulation of the nerve trunk, while not bearing against the present theory, as was mentioned before, may also be accounted for by the fact that, there being no effector upon which the more pronounced effects of asynchronization may be noted, one or more reversals of the dominance, due to refractory phase, of various fibers may occur before the considerable impairment of the total current.

Other Factors in Decrement by Repetition.—Although it is suggested that asynchronization lies at the core of all decrements in extinction and in negative adaptation, it is by no means denied that in some particular situations studied additional processes may have entered. In a great deal of the work on experimental extinction, the intervals between successive applications were so small that fatigue may have contributed to the decrement.[16] It is also possible that in the salivary reflexes there is some differential activity of the alimentary canal depending upon whether food is or is not taken in. Some inhibiting influence of the digestive tract is suggested by Boldirev's statement that he did not get any decrement upon repeated administration of the same food to deoesophagized animals, and one is tempted to appeal for such a function to the alimentary endocrines; but, of course,

[16] Without going into further discussion, it would seem to the writer convenient to confine fatigue decrement to short intervals of successive activation, where the change is transient and probably largely chemical—toxic and trophic—and to reserve modifiability decrement to longer intervals where the change is more permanent and largely neural.

such an appeal is not at present supported by any known evidence.

Restoration of Extinguished C–R's and Negatively Adapted to Reactions.—As the change produced in the various fibers and their branchings is subject to disuse, it would be expected that, with a lapse of time, the greater diversity among the fibers would be levelled out so that they approach their original states, and that, consequently, the completely or partially extinguished *C–R*'s would reappear or increase respectively. Furthermore, as the change produced by extinction is more recent than that by conditioning, it would be reversed in a proportionally greater degree than the latter, although it is of course true that time not only restores extinguished *C–R*'s but also wears away established *C–R*'s.

Dominance as Accounting for Special Facts of Conditioning

Generalization.—Generalization of *C–R*'s is well accounted for by irradiation, which apparently acts in two ways. First, there is considerable irradiation from the focus of the active conditioned stimulus, s, to the foci of the similar inactive stimuli, s_1, s_2, $s_3 \cdots s_n$, so that there are established not only the main pattern s–O–R, but also patterns s_1–O–R, s_2–O–R, s_3–O–$R \cdots s_n$–O–R, the strengths of which fall off as their anatomical and functional contiguity with the active conditioned stimulus becomes more remote. Secondly, there is irradiation from the foci of similar to the active stimulus so that the main pattern s–O–R is also activated in some degree by the application of the similar stimuli. That generalization, which in well established *C–R*'s is greatly narrowed down, is sometimes preceded by a period of less generalization, may apparently be explained by the fact that in the earliest stages of training the main pattern s–O–R is so weak that the still weaker s–O–R's of the similar stimuli are subminimal.

Differentiation.—That the narrowing down of the conditioned field of action is well explained by the Theory of Dominance, indeed, only by dominance, was pointed out before. In the usual experiments on obtaining differential

thresholds, not only reinforced applications of the active stimulus, but also non-reinforced applications of the inactive stimuli, are given, which, by extinction, break down further the weak connections of the adjacent stimuli with the conditioned response.

Conditioned Delayed and Trace Reflexes.—These *C–R*'s reveal two new interesting phases of conditioning: first, that two or more patterns may enter into a functional connection when a considerable interval of a few minutes intervenes between the beginning, or even the cessation, of the application of the stimulus of the first and that of the second pattern; second, that this interval tends to be constant by training, even though the second stimulus is not applied. The first phenomenon, while by no means suspected from the extreme brevity of irradiation as studied by the galvanometer, could be taken, however, as nothing else but undoubted indirect evidence of the long after-effect of excitation. The second phase will be considered here as a case of the well-known differentiations of intensities of stimuli, for, as it may be seen from numerous experiments, conditioned trace and delayed reflexes behave entirely as weak conditioned simultaneous reflexes, their strengths being roughly in inverse proportion to their intervals. The connection in such reflexes is then not just between patterns *S–O–R* and *s–O–r*, but between the former and a particular intensity of *s–O–r*, let us say, as the latter is in action two minutes after its initiation.[17] In the early stages of training, these reflexes are simultaneous or the delay is inconstant, because they have not as yet been differentiated and are therefore aroused by stimuli of different intensity, while later, as the differentiation grows finer, the delay becomes greater and more constant. The fact that strong conditioned stimuli tend to generalize the delayed or trace reflexes, is apparently because increasing the intensities of the conditioned stimuli raises the dominance of the immediate *s–O–r* more than that of the delayed *S–O–R*, so that the range of optimum dominance relationships between the

[17] Trace reflexes are not to be confused with time reflexes; the conditioned stimulus in the latter is probably organic.

two patterns is sooner overstepped than in simultaneous *C–R*'s.

Relationships between C–R's and Other Stimuli.—If some extra stimulus is applied to the organism either some short time before or during the action of an established or partly or completely extinguished *C–R*, various modifications resulting in an increase, decrease, reappearance, or disappearance of the response may occur. To account for these modifications various mechanisms have been assumed: external inhibition, inhibition of inhibition, static and dynamic irradiation, positive and negative induction. The value of all these terms will perhaps be questioned, if one keeps in mind that, according to dominance, there are always in conditioning two competing patterns, s–O–r and s–O–R, one of which is dominant and the other subordinate but still having a subminimal charge, and that the extra stimulus will, depending upon a number of factors, aid or hamper either or both of the patterns. Let a partly extinguished conditioned pattern s–O–R be activated, and a strong extra pattern S_s–O–R_s be given during its action, the strong S_s–O–R_s may be more dominant than the acting patterns, and the *C–R* will be absent; on the other hand, a medium extra pattern S_m–O–R_m may increase the propagation into s–O–R and thus increase the response, while another extra pattern s_w–O–r_w, which is weaker but perhaps more similar to s–O–r, will increase irradiation in the direction of the original response of the conditioned stimulus, and again no *C–R* will be produced. Similar situations will occur in applying an extra stimulus during the inactive phase of a conditioned delayed or trace reflex as well as during or before the action of an active *C–R*. In general, both experimental results and theoretical considerations indicate that the magnitude or even the direction of the effect of an extra stimulus is at present mostly unpredictable. One would have to know not only the conditioning and extinction history of each of the patterns involved, but also the similarity and dominance relationship of the extra pattern to both the conditioned and the original patterns of the conditioned stimulus. But the difficulty of prediction and the

multiplicity of factors do not call for additional hypotheses and can be well taken care of by those already given.

Conditioning and Higher Forms of Learning.—Whether learnings of the type of running mazes and solving puzzles, typewriting and reciting poems, fitting tubes into one another and getting out of problem boxes, proceed exclusively according to the known facts of conditioning, will perhaps be finally settled by experiment. It should not be impossible to set up complex situations, the constituent units of which are definite and limited, to observe the behavior of the organism towards the units and towards the total situations, to correlate such behavior with its expectancy from the known facts of conditioning, and finally to check up on the entrance of some additional mechanism. Meanwhile, however, the facts of conditioning do afford a complete account of the possible modifiable processes of these higher forms of learning. Bicycling, poem reciting, typewriting, and the like are adequately accounted for by proprioceptive and chained conditioning in which the response, or rather the proprioceptive part, of the first pattern becomes conditioned to the response of the second pattern, the response of the second to that of the third, and so on, this type of conditioning having been proven experimentally by many, especially by Beritoff. As to trial and error learning, previous accounts of it in terms of conditioning have stressed only one aspect of the problem, the elimination of the wrong responses, and also have not shown, except in a very general way, how the elimination might proceed. It need therefore be pointed out here that the elimination of wrong responses could be brought about, according to existing knowledge of conditioning, in three ways. The wrong response may be eliminated by its stimulus S_a becoming conditioned to a following response R_b, as in ordinary conditioning; it may be stopped, as in 'conditioned inhibition,' by the presence of another stimulus S_x so that $S_a + S_x$ will not give response R_a; and finally it may disappear by losing its dominance by repetition, as in extinction and in negative adaptation. To illustrate the last and probably most important way of elimination. Let situation 'S' be

connected in varying degrees to responses R_a, R_b, $R_c \cdots R_n$. Response R_a will for some reason be dominant over the other responses and tried first for one or more times; these trials will, according to our second generalization, weaken its connection and it will lose its dominance to response R_b, which in its turn, will lose its dominance by repetition to response R_c, and so forth, until finally the right response R_n is tried. The right response has now become dominant over all its preceding responses, the stimuli of which are now in varying degrees conditioned to it, strengthening it further. This weakening of the wrong responses by repetition and strengthening of the right response by conditioning are subject to disuse, but become more and more pronounced with each trial of the entire situation. In general, the fact that the successful response is the last is of great importance, not because of any recency value, but because in conditioning as also in spinal reflexes, succession in time is a potent dominance determinant, as seen in that an electric shock may become conditioned to a metronome, if the latter follows it in time (Beritoff).[18] In conclusion it may be remarked that the more factual objections of the 'Gestaltists' to trial and error learning, on the ground that it is not a particular but a 'kind' of movement which is acquired and that there is some transfer to other situations, are invalid, for this is just what would be expected from learning by conditioning. In as much as *C–R*'s are more or less generalized, on both the afferent and the efferent side, the learned act will be a 'kind' and will exhibit some transfer.

Summary

1. Organismic modifiability, known as 'learning,' 'conditioning,' 'adaptation,' 'extinction,' 'facilitation,' 'inhibition,' is held to be a result—apart from mere modification by stimu-

[18] This account does not intend to deny the importance of physiological states, behavior sets, 'rewards' and 'punishments,' in accelerating and retarding learning. These factors are undoubtedly effective in modifying general activity level, in raising or lowering the dominance of one or another response, and in bringing into play new reaction-patterns, but their effect does not seem to offer any specific problem in the mechanism of the elimination and selection of responses.

lation—of unequal rates of action and unequal modifications of rates of action of organismic units and groups of units of unequal functional states (Theory of Dominance and Theory of Asynchronization by Dominance).

2. Evidence both from organismic behavior and from neural action is brought forward in support of these theories.

3. Specifically, all dynamic behavior modifiability may be resolved into: (*a*) absolute improvement of the functional state of each elementary organismic unit by stimulation; (*b*) decrement of the response of a reaction-pattern by its repeated isolated activation; (*c*) increment of a response of a more dominant pattern by its simultaneous or closely successive activation with a less dominant pattern; (*d*) decrement of a response of a less dominant pattern by its simultaneous or closely successive activation with a more dominant pattern; (*e*) mutual decrement of the responses of patterns of close degrees of dominance by their simultaneous or closely successive activations; (*f*) substitution of a response of a less dominant pattern by a response of a more dominant pattern and the increment of the substituted response by repeated simultaneous or closely successive activations of the two patterns; (*g*) weakening of any modification by disuse.

4. No recourse to teleological concepts, on the one hand, and to 'internal' inhibitions, on the other, need be had.

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THE RELATION BETWEEN LEARNING TIME AND LENGTH OF TASK

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The present study was undertaken in order to test two hypotheses that the writer has formulated concerning the quantitative aspects of the learning function. These hypotheses were written in connection with the derivation of a rational equation for the learning function which is to be published in a separate paper.[1] The two hypotheses involved in the present study are as follows.

The relation between the number of items in a list to be learned and the learning time should follow approximately the following law:

$$T = \frac{c}{k} n\sqrt{n - a}, \qquad (1)$$

in which T is the total learning time, k is the learning constant of the subject, c is a constant depending on the degree of perfection required for experimentally complete learning, n is the number of items in the list, and a is the adaptation constant or attention span of the subject. Since the constant a practically disappears in significance for rather long lists of syllables or digits the above hypothesis states that the learning time varies approximately as the 3/2 power of the length of the list to be learned. This hypothesis has been formulated on the basis of theoretical considerations and on the assumptions underlying the derivation of a rational equation for the learning function. The hypothesis is limited to meaningless material so that it may not apply to the learning of prose or poetry or other meaningful material.

The second hypothesis that appears in the same analysis is that the relation between the number of repetitions and the

[1] To be published in the Journal of General Psychology.

number of items in the list should follow approximately the following law:

$$R = \frac{c}{k}\sqrt{n - a}, \tag{2}$$

in which R is the number of repetitions and the other notation is the same as before. Since both of these equations have been written from purely theoretical considerations it is not to be expected that they should fit data that have been collected under all conceivable conditions but the agreement between the theoretical equations and the available learning data for individual subjects indicates that the hypotheses are at least not far from the true relations.

When a series of individual learning curves are combined into an average learning curve for a whole group of subjects, there are inevitable distortions that our hypotheses do not cover. The hypotheses apply principally to learning data that have been collected for a single subject who has had sufficient practice before the actual experiment so that practice does not seriously affect the relation between learning time and length of the list to be learned.

In Fig. 1 we have plotted the relation between the number

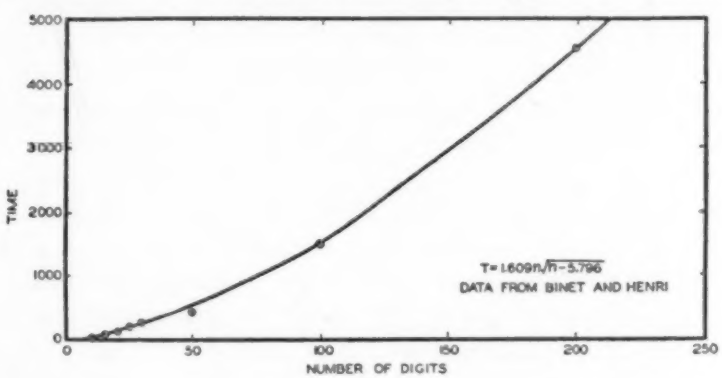

FIG. 1.

of digits and the total learning time according to some data from Binet and Henri (*1*). The smooth curve represents the equation 1 which has been fitted to the data. This equation, written in numerical form for these data, is as follows:

$$T = 1.61n\sqrt{n - 5.8}.$$

The adaptation constant is 5.796 or nearly six digits. It can be interpreted as the number of digits that the subject could recall correctly in perception time which is of course very small in comparison with any measurable learning time.

It seems that the spanning of any presentation perceptually whereby the subject can recall the whole presentation in the time which is required merely to perceive it is of a very much smaller order of magnitude than the time intervals that are measured in the form of practice. From the standpoint of practice or learning time, the time required to master five or six digits is practically zero. It is as though the learning curve began with an appreciable amount of proficiency at almost zero time which can be attributed to a perceptual function rather than to learning or practice proper. When the task is short or when the complexity of the task is of a low order, then it is mastered by the subject perceptually rather than by what could be more properly called practice or learning. Undoubtedly the perceptual function is continuous with the learning function but the perceptual function is so much more rapid than the learning function that for practical purposes the amount of material that the subject can grasp perceptually appears as though it were achieved in practically zero learning time. This span we shall call the adaptation constant or attention span of the subject. It can be either positive or negative. If, for example, the subject spends some time adjusting to the experiment and in discovering its plan or the exact nature of the task, then the adaptation constant might be negative. Ordinarily, however, it is positive and in that form we shall refer to it as the subject's attention span. It is the amount that he masters in practically zero time so far as the learning curve is concerned.

Inspection of Fig. 1 shows immediately a surprisingly good agreement between the theoretical curve and the experimental values. It is, in fact, much better than the writer had expected. The attention span of about 5.8 digits is the x-intercept of the curve. For large values of n, that is, for long lists of digits, the effect of the attention span may be

almost neglected. For these longer lists the function can be stated approximately in the form

$$T = \frac{c}{k} n^{\frac{3}{2}} \tag{3}$$

with the reservation that this approximation cannot be expected to apply to short lists. The entire range of the data is well satisfied by the general form of Equation 1.

One of the best experimental studies in this field is that of Lyon who has worked on different kinds of learning material and with different lengths of material (**2**). We have analyzed his data so far as it applies to meaningless material for both digits and nonsense syllables and for both of his methods of learning, namely the once-per-day method and the continuous learning method. In Fig. 2 we have repro-

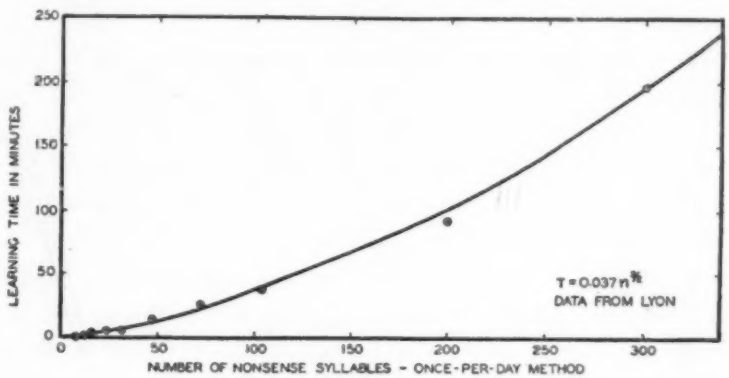

FIG. 2.

duced Lyon's data for nonsense syllables learned by the once-per-day method. Here the adaptation constant is so small in relation to the long lists that he learned that the constant may be neglected. The inclusion of the constant for a long sweep of data makes no appreciable difference in the goodness of fit when considered over the long range of lists up to three hundred syllables. The experimental data are represented by the small circles and the theoretical equation by the smooth curve. Inspection of this figure shows immediately that the agreement is quite satisfactory.

In Fig. 3 we have the data from Lyon's experiment with nonsense syllables by the continuous method. The experimental readings are again represented by small circles while the theoretical equation is shown as a smooth curve. Here

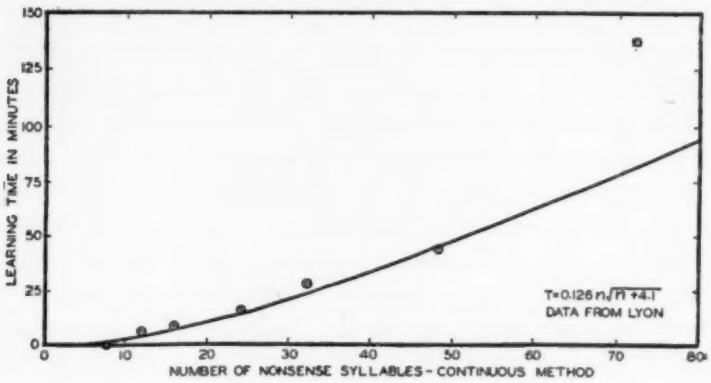

FIG. 3.

we have included the adaptation constant which is in the vicinity of four syllables. The theoretical curve fits fairly well all but one of the experimental observations. The learning time for 70 nonsense syllables, as reported by Lyon,

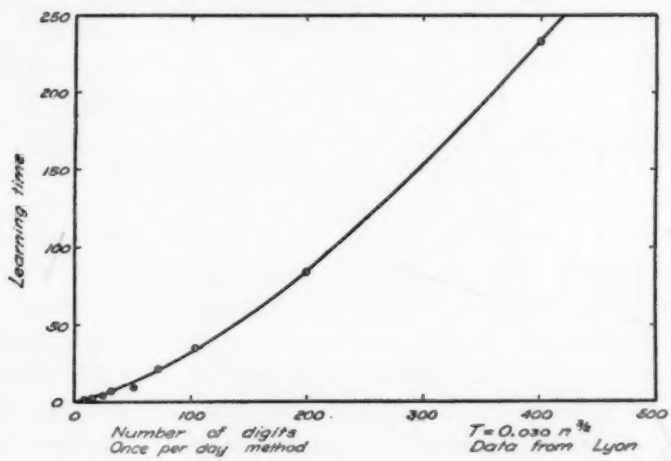

FIG. 4.

is considerably higher than that which would be predicted by our theoretical equation. This is the most serious discrepancy between theory and experiment in the data that have been

investigated so far. The theoretical curve fits satisfactorily the rest of Lyon's observations for the learning time of nonsense syllables by the continuous method.

In Fig. 4 we have the data from Lyon on the learning of digits by the once-per-day method. In this case the adaptation constant was ignored because of the long range of the experimental data. The equation gives a remarkably satisfactory agreement with the experimental observations. In Fig. 5 the data from Lyon on the learning of digits by the

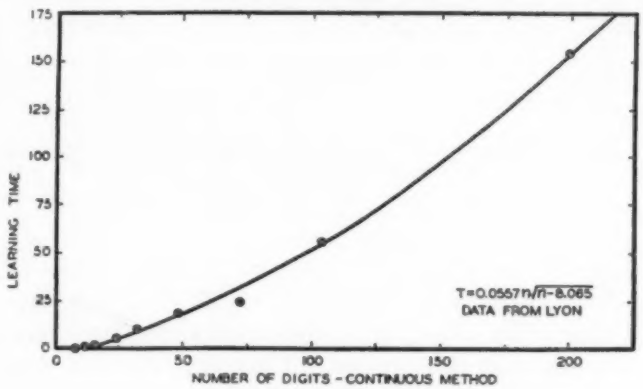

FIG. 5.

continuous method are similarly represented. The experimental observations are recorded as small circles and the theoretical equation 1 by the smooth curve. Here the attention span of the subject was found to be about eight digits. The theoretical curve agrees very well with the experimental data reported by Lyon.

The second hypothesis concerns the relation between the length of the task to be learned and the number of repetitions that are required to learn it. This hypothesis is represented by equation 2 above. One of the best known experimental observations on this problem is of course that of Ebbinghaus. In Fig. 6 his experimental data are plotted in the form of small circles and our theoretical equation is drawn as a smooth curve. The equation fits the experimental data quite well. The only discrepancies are found in the two shortest lists of 7 and 12 syllables for which the theoretical equation gives

only an approximate agreement. For the three longer lists the agreement between theory and experiment is perfect. It is quite probable that our rationalization of the adaptation constant is only a rough approximation so that perfect

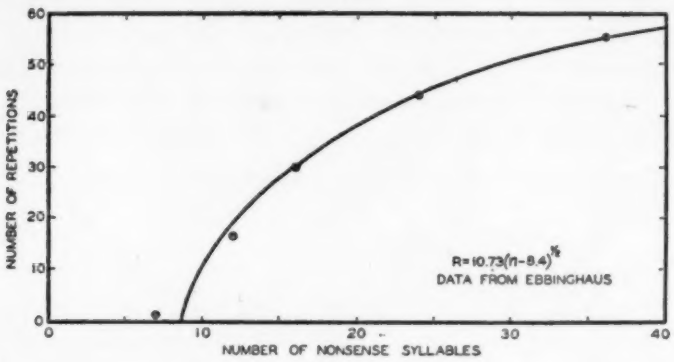

FIG. 6.

agreement for the short lists near that of the attention span is not to be expected. The data of Ebbinghaus agree with our hypothesis in that the number of repetitions increases as the square root of the number of items in the list with a correction for the attention span of the subject. In the data

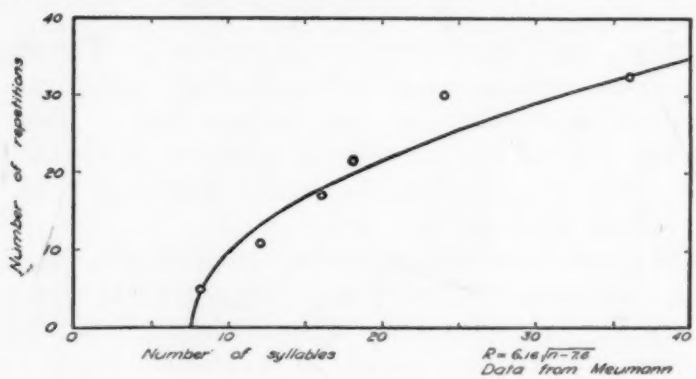

FIG. 7.

from Ebbinghaus this attention span is about 8.4 nonsense syllables which agrees fairly well with his own statement that he often could memorize eight syllables in one repetition.

Figure 7 shows the experimental data of Meumann, which

are quite unsatisfactory in that they show no definitely continuous function at all (3). It looks as though he had not continued his experiments under uniform conditions long enough to get a continuous function in his experimental data. All but his longest list might even be fitted with a straight line but such a fit would be discontinuous with the observation for his longest list. Consequently it is impossible to prove anything by any curve that might be drawn through Meumann's data. It has been included here with what seems to be the best fitting form of our equation 2. One conclusion can certainly be drawn from the data of Meumann, namely that the number of repetitions increases as some power of the number of syllables in which the power is less than unity. This main feature of his findings agrees with our theoretical equation according to which the number of repetitions increases as the square root of the number of syllables, corrected for the attention span of the subject.

The data of Lyon and of Ebbinghaus show more satisfactory continuity in the experimental data. In Fig. 8 we

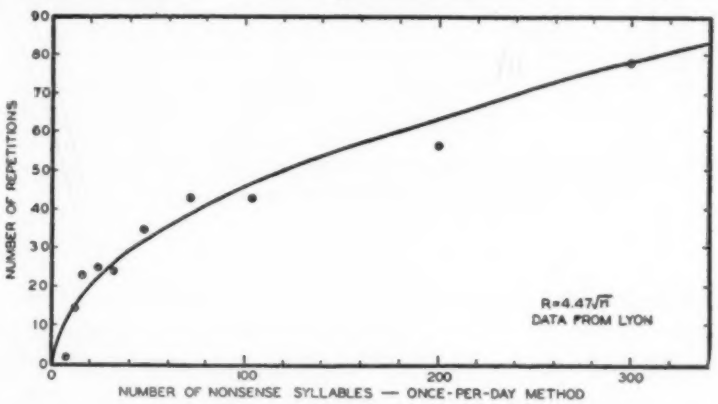

FIG. 8.

have Lyon's data for the learning of nonsense syllables by the once-per-day method. In the present case the data are arranged in the form of repetitions required for complete learning rather than in the form of total learning time. The attention span seems to be about seven or eight syllables but it has been ignored in writing the equation. The agreement

between the theoretical equation and the experimental date of Lyon is quite satisfactory and well within the range of variation of the experimental data themselves. A slightly better fit would be obtained for this set of data if the exponent were about .4 instead of .5 as required by our theoretical formulation. However, a discrepancy between a best fitting exponent of .4 and a theoretical exponent of .5 is well within the range of expectancy. It might be explained by the approximation that is involved in our rationalization of the adaptation constant. In the general characteristics of the curve the agreement between theory and experiment is quite satisfactory although not so striking as in the case of learning time and length of task.

Figure 9 shows the experimental data from Lyon on the

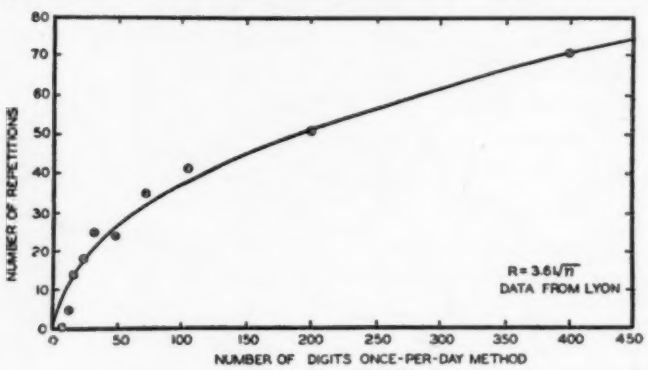

FIG. 9.

learning of digits by the once-per-day method. Here the agreement between our theoretical equation and the experimental data is rather striking. Certainly the general features of the experimental data agree with the theoretical curve. A slightly better fit would be obtained here also by a curve with an x-intercept of 7 or 8 and with an exponent of about .4 instead of .5 as required by our theoretical equation. The writer believes that an improvement can be found in the rationalization of the attention span which will bring about an even better agreement between theory and experiment especially for the experimental data on number of repetitions.

Both hypotheses give fairly good approximations to the experimental data.

The present study seems to warrant the conclusion that the learning time for meaningless material varies approximately as the 3/2 power of the number of items in the list to be learned except for short lists where a correction is made for the attention span of the subject. This law, including an approximate correction for the attention span of the subject, is represented in equation 1 above. The data also seem to warrant the conclusion that the number of repetitions required for learning a list of meaningless items varies approximately as the square root of the number of items in the list. Here also it is necessary to make a correction for the attention span of the subject when the law is applied to short lists that are of the same order of magnitude as the attention span. This law has been formulated algebraically in equation 2 so as to include an approximate correction for the attention span of the subject. Both of these laws have been set up as rational equations in a separate paper on the derivation of a rational equation for the learning function.

References

1. Binet, A., & Henri, V, La memoire des mots, *Année Psychol.*, 1894, 1, 1–23.
2. Lyon, D. O., Memory and the learning process, Warwick and York, Inc., 1917, Chapter III, Plates I, II, V, VI.
3. Meumann, E., The psychology of learning, John Wallace Baird, translator, D. Appleton and Company, N. Y. and London, 1913, page 276.

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GESTALT IN READING CHINESE CHARACTERS

BY SIEGEN K. CHOU[1]

Stanford University

Being a brain is not to live in gladness
So long as conscious mammals rant and rave:
For thoughts askew provoke my cells to madness
And force me fast toward a luckless grave. [1]

* * * * *

Being a brain is not to live in gladness
So long as conscious mammals rant and rave:
For thoughts askew provoke my cells to madness
And force me fast toward a luckless grave. [2]

We have all had the experience of reading upside down as in the second passage [2]. The words do not *look* as they used to be and yet once in a while their familiar structures pop out as vividly as ever. To put it in *Gestalt* terms, we are reacting to a slightly different phenomenal pattern. Our phenomenal field, so to speak, is appreciably changed. But the once familiar, good, precise, intimate, and closed configurational structures *have appeared* in experience. In accordance with the laws of *Prägnanz*, 'closure,' and 'intimacy,' their *reappearance* is persistent in spite of the less favorable conditions.

Being a brain is not to live in gladness
So long as conscious mammals rant and rave:
For thoughts askew provoke my cells to madness
And force me fast toward a luckless grave. [3]

We appreciate this experience all the more by reading the above mirror printing [3] of the same passage.[2] Here

[1] The writer is indebted to Prof. W. R. Miles for experimental direction, to Prof. K. Koffka, Dr. Helson, and Mr. I. Huang for criticisms and suggestions.

[2] This passage in mirror printing is taken from the front-page Soliloquy of Brain in a curious little book entitled "The Ultimate Philosophy," 1925, by Jack Borlase.

we notice that the sequence of the individual letters and words is still the same as in the upright [1] and upside-down [2] position. In other words, units of print are always in the same succession. There is no alteration in the relative position of the constituent elements. Just as in looking at the object on the ground glass of a camera or drawing the star in the mirror, we adjust ourselves to the novel situation once for all and the objects yield to familiar interpretation without much further annoyance. This orientation of the reading material as a whole without altering the relative position of its constituents we shall call an *orientation of the first order.*

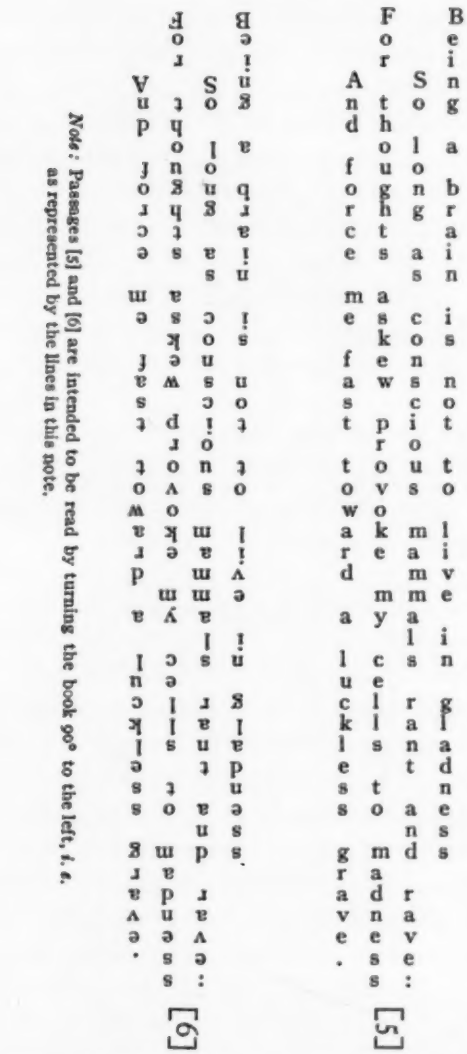
Now instead of turning the entire word and page as a whole upside down, suppose we print separately the individual letters upside down and read in the normal manner. What do we experience?

[4]

ǝpısdn ʇou ɹɐ ʇsɐ ɹǝʌǝ ɹɐ ʇɐɥʇ ɯɐɹ ɐ ƃuıǝq
:ǝʌɐɹ ɟo ʇuǝɯɐɯɯɐ ʇuǝıɔuoɔ sɐ ƃuol os
ssǝuɟɐq ʇo sllǝɔ ʎɯ ǝʞoʌoɹd ʍǝʞsɐ sɥʇnoʇ ɹoℲ
.ǝʌɐɹ sǝllʞɔnl ɐ ɹɐʍoʇ ʇsɐl ǝɯ ǝɔɔoɹ ɟo ʇuǝɯ ɐ

The above passage [4] is exactly the same as before in the mirror [3] and upside-down [2] printing except that the sequence of the individual letters (although not words) is changed by turning them upside down *separately.* In other words, units of print are no longer in the usual succession. There is alteration in the relative position of the constituent elements. The different words as a whole do not *look* like themselves so much as when they are either upside down or in the mirror. The word-*Gestalt* has been changed, losing all its characteristics as such. We not only have to orientate ourselves all the time to the general upside-downness of the entire reading material but also are forced to figure out individually and without exception the identity of every word-*Gestalt* as it used to be. In short, it requires much more effort to understand what we are reading.

We can still turn the individual letters only half way round, *i.e.*, tilt them 90° either to the left [5] or to the right [6].


Being a brain is not to live in gladness
So long as conscious mammals rant and rave:
For thoughts askew provoke my cells to madness
And force me fast toward a luckless grave.
[5]
Begin a quarrel is not to live in sadness
So long as conscious mammals rant and rave:
For thoughts awake my cells to joy and gladness
And force me fast toward a luckless grave.
[6]

Note: Passages [5] and [6] are intended to be read by turning the book 90° to the left, *i. e.* as represented by the lines in this note.

Notice how the form of every word is changed! Words in the second passage do not *look* like themselves at all but they are much more meaningful in the first passage as we tilt our head a little toward the left in reading them. This secondary word-*Gestalt* is still kept intact and is best appreciated by tilting our head in conjunction with it. This orientation of the reading material by altering the relative position of its constituents we shall call an *orientation of the second order.*

Generally it can be said that, so long as the sequence of individual letters and words is normal as in the usual printing of all Western languages, *i.e.*, *left-follows-right*,[3] the word-*Gestalt* suffers little distortion from an unusual orientation of the first order, *i.e.*, by turning the entire page as a whole upside down or viewing it in the mirror. Therefore it presents little difficulty for reading no matter in what direction we read. In every case the individual word as a whole has not been changed; its relative position on the page is still the same. As soon as the sequence of the individual letters and words is changed to *right-follows-left* from an unusual orientation of the second order by turning the individual letters independently upside down [4] or to *bottom-follows-top* from a similar orientation by turning the individual letters independently 90° to the right [6], the word-Gestalt is hopelessly distorted and hence presents difficulty for reading. However, the *top-follows-bottom* sequence produced also from the second order orientation by turning the individual letters independently 90° to the left [5] seems to be an exception, because this sequence being semi-normal as used in vertical signboards yields a secondary word-*Gestalt* that is easily appreciated by compensatory head movements.

The above analysis of the English alphabetical word-*Gestalt* as affected by unusual orientations of different order is not so much an attempt to call attention to what is perhaps already obvious and common place as an advance toward the recognition and appreciation of an analogous situation in the case of even a *sentence* of Chinese characters. As probably all know, every Chinese character is a little symbol made up of different kinds of separate strokes in various arrangements, not unlike a piece of architecture having its own unity and coherence. This unity and coherence give a distinct form to every character that is different from all others. It would be absurd, of course, to rearrange the different strokes because that would be equivalent to making a new word. But we can instead put the whole square character in different

[3] Here the terms 'left,' 'right,' 'top,' and 'bottom' refer to the sides of individual upright letters or words, in the order that they are reached in reading.

positions as we did above for the English alphabet. We purposely turn the characters upside down and arrange them in four ways, namely, one above another, one below another, one at the right of another, and one at the left of another. Corresponding to each and every one of these four arrangements, we ask a group of Chinese to read downward, upward, rightward, and leftward respectively. We repeat the same experiment also with characters tilted 90° to the right and characters tilted 90° to the left. After all these results are obtained, we make a comparison with those obtained in reading upright characters in the various directions. What would be the effect on speed or efficiency of reading? Would it be, if any, as great as it seemed to be in altering the positions of the individual letters composing the English word as shown above? Can we account for the effect, if there be any, by postulating an analogous sentence-*Gestalt* for Chinese characters as we have shown for the English *word?*

There seemed to be little sense in performing the experiment on English as illustrated above but I thought it would be worthwhile to do so in the case of Chinese characters. The fact that Chinese characters are uniform squares and hence can be arranged either vertically or horizontally has led people during the last decade to adopt the practice of horizontal printing and writing. As to the comparative speed of reading in the two arrangements, most experimental findings [4] seemed to show that vertical reading is faster. This is of course with reference to downward and rightward readings. Upward and leftward readings have never attracted the attention of other experimental psychologists. I actually carried out early in 1928 the experiment [5] suggested above, comparing the relative speed of reading Chinese by a new reading reaction technique.[6] The reading material

[4] H. T. C. Tu, The effect of different arrangements of the Chinese language upon speed and comprehension of silent reading, *Chinese Soc. & Pol. Sc. Rev.*, 1926, 10, 278–301. E. Shen, An analysis of eye movements in the reading of Chinese, J. Exper. Psychol., 1927, 10, 158–183.

[5] S. K. Chou, Reading and legibility of Chinese characters: I. The influence of reading-direction and character-position upon speed, J. Exper. Psychol., 1929, 12 156–177.

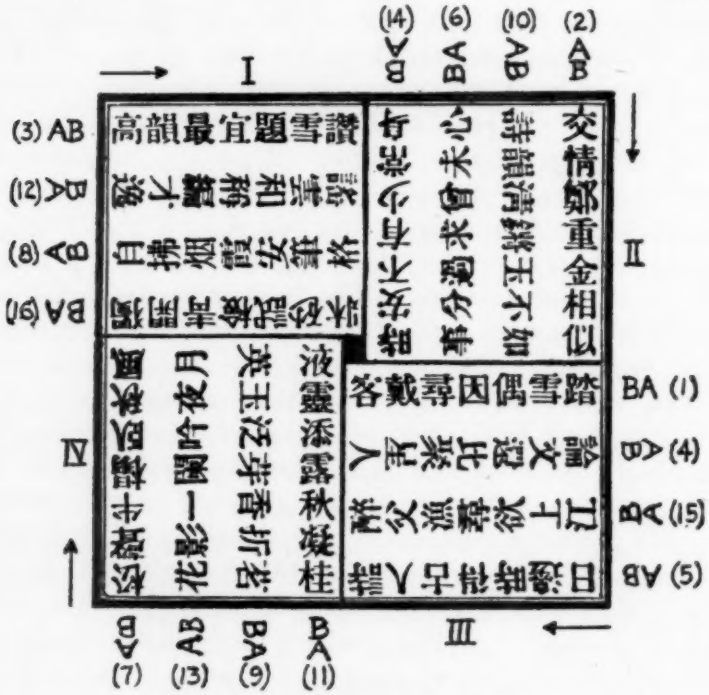
[6] S. K. Chou, Reaction keys and a new technique for reading-reactions, *Amer. J. Psychol.*, 1929, 41, 469–473.

in sentences were pasted on separate cards and exposed one at a time behind the round window of a specially designed *Quadrant Tachistoscope*[7] or *Bradyscope*, which has four shutters pressing against one another in the form of a cross. The subject opened the window himself by pressing a key, read aloud what was exposed and then let the window close itself by releasing his finger from the key.

The usual method of early research in reading speed has been to obtain the total time in reading a passage and then to find the average rate in terms of words read per second by dividing the total number of words by the total time. The efficiency of reading is thus noted in terms of amount done in unit time rather than time required for reading a unit amount of material. The departure of the present method is just the reverse of the ordinary procedure, by taking sentences of seven characters as unit amount of material to be read and recording the reading time for every unit amount. The theoretical justification for this procedure is fourfold. (1) The time required to do a given amount is a much more adequate and accurate measure than the amount done in a given time, because there is no limit in the time that one may acquire to accomplish a given amount of work while there is always a certain minimum amount of work that one can accomplish. (2) The absolute decrease in number of words read per second can not be taken as a direct measure of the effects of different factors that influence the reading speed in different individuals because this decrease is the least in the slowest reader while his increase in reading reaction time is the greatest. (3) This procedure makes possible the collection of a large number of reading reaction times for statistical treatment in evaluating the different effects of various factors that influence the speed of reading Chinese, in view of the fact that the representative measure in terms of words read per second is statistically not sound, since it is in no sense a central measure of any distribution of separate measures (unless by photographing eye-movements) but an abstraction in artificially divisible units from

[7] S. K. Chou, A quadrant tachistoscope for studying the legibility of Chinese characters, J. Exper. Psychol., 1929, 12, 178–186.

a single measure of larger unit. (4) The *simultaneous voluntary maintenance* of the exposure of the tachistoscope for the reading reaction is at the same time a non-discrete detractor, a constant check, and a continuous intensifier of the attention of the reader.[8]



I is for reading from left to right
II is for reading downward
III is for reading from right to left
IV is for reading upward

The positions of 'AB' represent the corresponding positions of all the characters in the sentence and the orders of 'AB' represent the directions of reading. The figures in parentheses are rank orders of reading speed.

FIG. 1.

A sample of the reading material is schematically shown in Fig. I. The results of the experiment are rather unexpected. Leftward reading is about 5 per cent faster than rightward reading. Moreover, leftward reading is about 4 per cent faster than even the traditional downward reading.

[8] S. K. Chou, Reaction keys and a new technique for reading-reactions, *Amer. J. Psychol.*, 1929, 41 469–473.

Rightward reading of upside-down characters has the slowest speed, about 20 per cent slower than leftward reading of upright characters and about 17 per cent slower than downward reading of upright characters.

The problem of the comparison of speed in vertical and horizontal reading was first investigated by Huey.[9] He did not, however, alter the relative positions of individual letters within the single word. This keeping intact of the characteristic form of the individual words so far as the relative positions of the constituent letters are concerned, is sufficient to account for the fact that "with sense matter the vertical reading was only from seven to ten per cent slower." [10] By using words of a uniform number of letters (from two to sixteen), he remarked that "it is interesting to observe that while the shorter words are read more rapidly in horizontal sequence, the longer are read more rapidly in vertical." [11] This tends to show that perhaps the longer the word the more characteristic its form will be. Since the longer words of uniform length are one above another in a column and are perhaps more widely separated from one another between columns than within the same row, it is perhaps natural that their more characteristic forms stand out more clearly and hence render a slightly greater reading speed than shorter words arranged one beside another. The significance and importance of this characteristic form of a word may be appreciated by inspecting the following diagram corresponding to that shown above for a sentence of Chinese characters.

The total quality of the word 'CHINESE' (1) is not lost when it is tilted *as a whole* 90° to the right (2) or left (3) or even turned upside down (4). Although there is a tendency to tilt the head in the same direction the total word is tilted, this directional shifting of attention hinders very little the unusual spatial orientations for the different positions and the *Gestalt* of the word is instantly grasped. On the other hand, when the individual letters are placed separately in

[9] E. B. Huey, Preliminary experiments in the physiology and psychology of reading, *Amer. J. Psychol.*, 1898, **9**, 575–586.

[10] E. B. Huey, The psychology and pedagogy of reading, 1908, p. 425.

[11] E. B. Huey, *op. cit.*, p. 576.

the different positions, the characteristic form of the whole word is so hopelessly altered that it is more likely to be interpreted as, *e.g.*, 'ESENIHC' than 'CHINESE.' Whereas the greatest detrimental effect on reading speed in the case of a *sentence* of seven Chinese characters similarly altered is

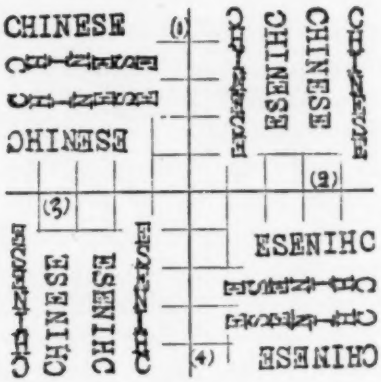


FIG. II

only 20 per cent, there are reasons to conjecture that the corresponding effect on the English *word* will be at least several times greater. In the first place, the unit of perception in a sentence of seven Chinese characters is the individual character itself, while an English word of seven letters is required to be perceived as a whole. This perception of the total word is a much more immediate process than the comprehension of the total sentence. The meaning of the sentence comes to light as the individual characters are read but the significance of the single word can not be understood as the individual letters are perceived, but depends more upon the total form of the word itself. Since this total form is lost in altering the position of the individual letters independently of the resulting word, it must be first restored either in form of auditory imagery of the sound of the word as gathered from the partial recognition of the individual syllables or by aid of visual rearrangement of the letters in their normal spatial relationship before the total word can be perceived as such. Thus the immediate perception of the total word is much

delayed. In the second place, a sentence of seven Chinese characters may be read singly with different degrees of comprehension of the meaning of the total sentence or even with no comprehension at all, but an English word of seven letters, if recognized at all, must be recognized fully. In the third place, although Chinese characters are perhaps better adapted for vertical arrangement so far as arrangement and nature of strokes are concerned, the fact that all of them are uniform squares occasions much less distraction when placed in the horizontal than English letters placed in the vertical, because the latter is higher than it is wide and this unduly lengthens the word when placed one above another. Strictly speaking, a *sentence* of seven Chinese characters is hardly comparable to an English *word* of seven letters. It would be absurd to attach equal importance to the *sentence-Gestalt* in a line of Chinese characters and to the *word-Gestalt* in any alphabetical language as affected by unusual orientations of the constituent elements. But the fact that there *is* an analogous *Gestalt* within the line seems to be fully established even in the case of a line of English words in Huey's experiment.

Huey had reasons to believe that "it seemed entirely likely that the vertical arrangement might ultimately give even greater speed than the horizontal" [12] in the case of English *words*. But in spite of all the hypothetical advantages, his result for vertical reading was still from seven to ten per cent slower. It is not enough to account for this by simply attributing it to the artificial pasting of the words and to the novelty of the vertical arrangement in general; [13] something intrinsic in the sequential order of eye-movement pauses, either up-down or crosswise during passage from word to word must be responsible for the apparent difference. In the normal crosswise rightward reading, the sequential order of eye-movement pauses is *along* the entire linear width of each word from word to word, whereas in the new downward reading, the sequential order of eye-movement pauses is *across* only a section of each word from word to word. The

[12] E. B. Huey, The psychology and pedagogy of reading, 1908, p. 425.

[13] E. B. Huey, Preliminary experiments in the physiology and psychology of reading, *Amer. J. Psychol.*, 1898, **9**, p. 579.

very fact that the *Gestalt* of the uniform linear nature of the reading material in a row is now changed into that of a *pile* of irregular lengths of words in a column is perhaps a more adequate explanation for the discrepancy. If there is any novelty at all, it is this novelty of the *Gestalt* of the line or sentence.

Words or letters follow one another. The word 'follows' implies a meaning that is both spatial and temporal. When two or more words or letters stand together on the page either side by side or one below another, they are only spatially following one another. But so long as they are intended to be read either rightward or downward and as soon as there is somebody actually reading them, they are not only spatially following one another but temporally as well. Referring to the top, bottom, right and left sides of a word or letter always in its upright position, it is evident that any side of one word or letter may be close to any side of the next. Thus, the temporal-spatial sequence in the usual rightward reading is right-side-to-left-side, *i.e.*, the perceptual process is *first from* the right side of one word or letter *and then to* the left side of the next. This may be represented by the two letters (AB). This sequence is undisturbed when the two letters are subject to an unusual orientation of the first order, *i.e.*, turned as a whole upside down (ꟼV), 90° to the right($\overset{A}{B}$), or 90° to the left ($\overset{B}{A}$). Similarly the temporal-spatial sequence in the Chinese normal downward reading is bottom-side-to-top-side, *i.e.*, the perceptual process is *first from* the bottom side of the first *and then to* the top side of the next, $\binom{A}{B}$, $\binom{ꟼ}{V}$, (ꟼ>), and (<ꟼ). However, in leftward and upward readings, the temporal-spatial sequences are just reversed respectively. Thus, in (BA) the sequence is left-side-to-right-side and in $\binom{B}{A}$ top-side-to-bottom-side. The same sequence of (BA) is found in (Vꟼ), $\binom{ꟼ}{A}$, and $\binom{B}{ꟼ}$, and that of $\binom{B}{A}$ in $\binom{V}{ꟼ}$, (>ꟼ), and (ꟼ<), although the general directions for reading are changed. It is clear then

that the temporal-spatial sequence is independent of either the word-position or the reading-direction. One sequence can be changed to another only by virtue of an orientation of the second order, *i.e.*, by altering the position of the constituent letters or words independently of one another. The general preference in reading efficiency is not for a certain absolute reading-direction nor for a certain absolute character-position exclusively but rather for a certain relative temporal-spatial sequence resulting from the direction-position combination. It is this temporal-spatial sequence that constitutes the *Gestalt* of a line of Chinese characters. The 'bottom-to-top' relationship is inherent, natural, and deep-rooted. Just as Köhler's apes reacted not to the food containers as such but to the situation 'darker than,' so the Chinese adult readers find their reading efficiency preference most strikingly for the primary *Gestalt* of 'bottom-to-top' sequence irrespective of the reading direction, primary in the sense that it had been the only traditional sequence for centuries until about a decade ago. Köhler's apes learned the 'darker than' situation; adult Chinese readers learned the 'bottom-to-top' sequence. It is not the downward direction as such that favors the speed but the natural temporal-spatial sequence of the characters resulting from it. On the other hand, $\binom{B}{A}$, $\binom{V}{B}$, ($\succ$), and ($\prec$) all rank pretty low because they are all read in the opposite temporal-spatial sequence of top-to-bottom, *i.e.*, *first from* the top of one character *and then to* the bottom of the next, which is very unusual. It is a poor *Gestalt* and is clearly shown by the fact that its reading speed is the slowest. Although the other two sequences do not differ much in rank order, the average time of the three unusual temporal-spatial sequences combined is .10 second or 5 per cent longer than the habitual 'bottom-to-top' sequence.

As set forth in an earlier report,[14] it is my contention that the problem of vertical and horizontal reading of Chinese can hardly be attacked conclusively at all by using adult subjects

[14] S. K. Chou, Reading and legibility of Chinese characters: I. Influence of reading-direction and character-position upon speed, J. EXPER. PSYCHOL., 1929, 12, 156–177.

under the assumption that the habit factor is equally effective in the two cases. As a matter of fact, we have no justification for making such an assumption unless we are perfectly certain that other differential factors can be adequately and independently elicited apart from the conditioning influence of the habit factor. In other words, the two manners of reading are *not comparable* in the first place. Both Tu and Shen, while admitting the possible rôle of habit in the explanation of their findings in favor of vertical *downward* reading, sought for differential factors other than habit. Tu stresses several physiological factors,[15] such as the relative positions of the two eyes, retinal field of clear vision, peripheral vision, lid and eye movement fatigue, muscular mechanism of the eye-balls as well as the possible factor of the common illusion of vertical and horizontal lines, as support for his view that "other things being equal, the horizontal arrangement is more efficient than the vertical" [16] and for his finding that horizontal reading of geometrical figures is faster. But Shen tends to refute all of them [17] and adds further that asymmetrical convergence and accommodation and the greater facility in executing eye-movements of small magnitude in the vertical are all against horizontal reading. The question is not how many possible differential factors we can enumerate to account for the difference in favor of one or the other, but whether it is fertile or futile to employ adult subjects—to compare what is in reality not comparable. Tu concludes, "The Chinese students read the Chinese material better in the vertical arrangement, because they had habitually practised that method." [18] But it does not follow that "if the vertical arrangement were really more efficient than the horizontal, the results should have been the same for reading the geometrical figures as for the Chinese material."

[15] H. T. C. Tu, *Chinese Soc. & Pol. Sc. Rev.*, 1926, 10, pp. 292–294. In an article entitled Why should we write horizontally? published in a small publication while in middle school as early as 1919, I noted most of these physiological factors.

[16] *Ibid.*, p. 298.

[17] E. Shen, J. Exper. Psychol., 1927, 10, pp. 178–182. Cf. E. B. Huey, Preliminary experiments in the physiology and psychology of reading, *Amer. J. Psychol.*, 1898, 9, p. 575.

[18] H. T. C. Tu, *op. cit.*, p. 298.

He admits the rôle of habit in the first case but denies it in the second. What he did prove is that faster reading of geometrical figures in the horizontal is due to the fact that geometrical figures are habitually read in *that* arrangement also by the Chinese students. They never encounter geometrical figures arranged vertically and it is improbable that the immense amount of practice in reading vertical Chinese should have so much positive transfer on reading horizontal geometrical figures as to yield quicker speed in the vertical.

The real point seems to be that habit and training have conditioned favorably both the reading of Chinese characters in the vertical and of geometrical figures in the horizontal. Chinese characters have been better adapted in the long run to the 'bottom-to-top,' (or top-*follows*-bottom), sequence for adult Chinese readers and geometrical figures to the 'right-to-left' (or left-*follows*-right), sequence for the same readers. If a squad of soldiers line up for roll call day in and day out from one end to the other, it makes a difference when they are suddenly ordered to resume the short-to-tall order instead of the tall-to-short. Mr. A recognizes now he is no longer at the right of Mr. B. but at his left, and so on. The neighbors of every soldier have been interchanged so that, should the reviewing officer have the habit of finding fault only with the right half of every soldier's uniform in his daily morning inspection starting from the left end of the line, he might be surprised to find now that the whole squad seems to be improperly dressed as he proceeds from the right end of the line this time. He is still paying attention to that side of every soldier as first encountered by his line of regard but in reality he is reviewing the left side of every one of them. An analogous situation exists in the temporal-spatial sequence of Chinese characters in which the saccadic movement of the eye during the perceptual process of reading traverses from one side of one character across the adjacent side of the next character. Reading efficiency is a function not of the units of print as such but most markedly of the units in temporal-spatial sequence and, what is more, of the past experience of the reader. It is the sum-total or rather *Gestalt* of the reading

units in temporal-spatial sequence, the phenomenal and reaction patterns and configurational dispositions and capacities within the reader that determines the reading efficiency. Shen was of the opinion that "though habit is a necessary and may even be a sufficient explanation, it is certainly neither exclusive nor exhaustive." [19] It is a sufficient explanation for all conclusions favoring Chinese characters in the vertical and geometrical or other materials in the horizontal for adult Chinese subjects because it affects the relative reading speed between not only downward and rightward reading of upright reading material but also other reading directions and other character positions as well. However, it can be at the same time *the* exclusive and exhaustive explanation in the sense that the age-long and life-long traditional practice in vertical downward reading of upright characters has entirely outdone, eclipsed, and obliterated the hypothetical influence of all other possible factors, as shown in my earlier report.

The total quality of the relative positions of characters in a column or row is more important and significant than the general direction of reading. The fact that reading leftward is faster than rightward and even downward is inexplicable by any differential physiological factor. It should be pointed out (and this seems to be the only plausible explanation) that, although the direction of reading leftward within the line as is not infrequently met with in posters or commercial signboards is neither that of the Western nor that of the Chinese original usage, this general direction is nevertheless the habitual direction of progress from line to line in the usual downward reading. In the normal downward reading, although the direction of eye-movement within the line is from top downward, the direction of head movements from line to line is from right leftward. The faster the reader, the greater significance should be attributed to this fact. It is a common observation that more head movements occur in fast readers who skim over the page 'ten lines in a glance' as the Chinese saying goes. The effect of the constant practice in this head movement during reading can not be underestimated. Furthermore, the habitual direction of progress

[19] E. Shen, *op. cit.*, p. 179.

from line to line is in a way also a secondary normal temporal-spatial sequence in the sense that the return sweep of the eye is *first from* the left side of one of the characters at the bottom of the first line *and then across* the right adjacent side of one of the characters on the top of the next line. When reading leftward in a line, the significance of this sequence is more marked and hence contributes to its favor in reading speed, not because the eye during the return sweep actually sees the adjacent sides of the characters between lines (which is not true) but because the perceptual process has a temporal 'before-and-after' as well as a spatial 'left-and-right.' It is this temporal-spatial totality that constitutes the *Gestalt* of a sentence or line of Chinese characters.

What actually conditions reading efficiency is not the reading direction alone nor the character position alone but also the temporal-spatial sequence created through the interaction of both. The normal habitual temporal-spatial sequence is 'top-follows-bottom,' or from the standpoint of the reader, 'bottom *to* top,' *i.e.*, *first from* the bottom of one character *and then to* the top of another, irrespective of either the character position or the reading direction. Perhaps this is true in all perceptions of successive objects of a pictorial nature. For instance, in viewing a picture the directional attention is perhaps more frequently up-down than crosswise. It is only a truism to say that the uprightness of a Chinese character determines this directional attention in the crossing-over from character to character. When unusual positions of characters are introduced, the upright position is always taken as a reference for interpreting them and the normal habitual temporal-spatial sequence has a much greater tendency to run into completion, so to speak, than all the other sequences.

This temoral-spatial sequence as hitherto defined is what I call *concretely* the *Gestalt in reading* Chinese characters. The invention, evolution, etymology and architecture of the Chinese characters themselves follow also definite configurational laws, which will be the topic of another discussion. When I am constantly reiterating the single concept of temporal-spatial sequence of the characters in group, I am cognizant of Krueger's recent criticism that "The danger

exists already that the name Gestalt will be used as a magical lamp for any psychological darkness, and especially that the misuse of such an important concept will prevent the completeness of *analysis* both of phenomena and of conditions." [20] *Gestalt* exists only *after* complete *analysis* both of phenomena and of conditions.

I have tried to show that in all Western alphabetical languages running from left to right the word-*Gestalt* suffers comparatively less distortion by turning the entire line or page as a whole (orientation of the first order) than by turning the individual letters independently (orientation of the second order) in different positions. An analogous sentence-*Gestalt* in reading Chinese characters may therefore be defined as the temporal-spatial sequence of the individual characters in line irrespective either of the reading direction or of the character position. As the Chinese characters are traditionally arranged one below the other and intended to be read downward, there has resulted in adult Chinese readers a primary temporal-spatial sequence, that is, the top of one character follows the bottom of the foregoing, which invariably yields a faster reading speed no matter in what position the entire sentence is turned or in what direction it is read. I offer this as *the* explanation of all former findings that adult Chinese readers always read downward the faster and have called attention to the futility of using such subjects for experiment because they have already been dominated by one particular temporal-spatial sequence. As an explanation of the particular result of my experiment that leftward reading is not only faster than rightward but even the traditional downward reading, I have pointed out the fact that in the original Chinese usage the general leftward direction of progress from line to line has unwittingly conditioned a secondary sentence-*Gestalt* in creating a semi-normal temporal-spatial sequence, that is, right side of one character follows the left of the foregoing, *in conjunction with* the general downward direction of reading within the line.

[MS. received June 29, 1929]

[20] F. Krueger, The essence of feeling: Outline of a systematic theory, Feelings and emotions, the Wittenberg Symposium, edited by M. L. Reymert, 1928, p. 66.

THE COMMON SENSE OF INTROSPECTION

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I. The Mobility of Conscious Behavior

The first difficulty that faces the psychological experimenter is the fact of everyday observation that his subjects do not all respond to the proffered stimulus in the same way, and that individually they do not respond in the same way to the same stimulus at different times. This variable phenomenon of response in the individual observer has been systematically generalized and dealt with in psychology under the term of attention. However, as the facts which have been gathered from a study of the phenomenon designated by the term attention have been subjected to objectionable generalizations in some quarters, it is perhaps more advisable to use the term conscious behavior, which refers to the same set of facts and which does not carry with it certain objectionable historical connotations that have accrued to the former term.

The common sense reason why individual stimuli do not secure uniform responses is that for the observer they blend into a continuous yet ever varying flow of conscious behavior. Incoming sensory events of importance are automatically adapted and represented in conscious behavior without seriously disturbing the general organic continuity. There is a continuity of function throughout the waking life of the normal being, and the response to a discrete stimulus depends upon the way that this external event merges into the flow of internal organic events.

If we set ourselves the task of listing the conditions which render it difficult to obtain comparable stimulus-response results with different observers or with the same observer at different times, we find that they are conveniently divided into the following classes: 1. Those which may be attributed to the

nature of physical patterns; 2. Those which may be attributed to the peculiar behavior of the sensory receptor employed; 3. Those which may be attributed to central conditioning of the incoming processes. The third set of difficulties is by far the most complex and involves physiological schemata, individual difference patterns, laboratory sophistication, familiarity with task and the general problem of analysis.

II. Physical Patterns as Conditioners

Up to recent years psychologists in attempting to classify conscious behavior have put the emphasis chiefly upon logical subjective patterns (sensations and perceptions) rather than upon objective physical patterns. A contribution of the *Gestalt* school has been to stress the possibility of classifying mental experiences upon the basis of objective patterns. An important advantage of this emphasis lies in the new basis that it gives for envisaging certain fundamental phenomena relating to the directive aspect of conscious behavior. Instead of basing the classification upon the function of attention, *Gestalt* psychologists have placed a much needed emphasis upon the types of situations as conditions which bring about varying kinds of representations in conscious behavior.

For example, by taking the functions of physical objects or events as a basis we can classify them in the following way:

1. Physical objects differ in respect to one another when the absolute values of certain aspects of these objects are changed. For example, form influences color, brightness influences form, contour and color, exposure time influences brightness, color and form.

2. Physical patterns may differ in the degree to which they compel a complete orientation of the organism.

a. Size of stimulus. Large stimuli are more insistent and impressive than small or moderate sized stimuli. In vision the relative size of the stimulus in terms of the visual angle subtended determines the degree of analysis possible. If the stimulus is too large the organism cannot grasp it *en toto.* On the other hand, if the stimulus subtends too small a visual angle the organism cannot, except by the use of physical

instruments, differentiate between the possible observable details.

b. Intensity. If the intensity of the stimulus is beyond a certain degree, the capacity of the sense organ is taxed to the extent of injury and the stimulus becomes insistent and impressive.

c. Change. The abrupt appearance or disappearance of anything in the sensory field causes a reorientation of the organism toward it. Any disturbance of the equilibrium of sense organ and conduction processes such as change of spatial position, or a qualitative change such as a variation of hue, pitch, timbre, etc., usually produces a reorientation or reorganization of conscious behavior with respect to the physical event.

3. Patterns may vary in their temporal relations in conscious behavior.

a. Patterns differ in respect to the time that they can keep the observer oriented toward them: *e.g.*, beautiful living organisms versus static unattractive, inanimate objects.

b. Patterns differ in respect to the time taken by all details to become organized into a homogeneous whole in conscious behavior: *e.g.* unfamiliar objects are differentiated and classified gradually. Familiar objects are differentiated and classified immediately.

4. Patterns may vary in the degree of homogeneity of the conscious behavior organization aroused.

a. Patterns may differ in symmetry. Symmetrical patterns are usually preferred to asymmetrical ones. Incomplete figures are often completed to make symmetrical ones.

b. Patterns may differ in the number of psychological centers or locuses involved. For example, a pattern may be without a *Schwerpunkt*, *e.g.* a large plain-colored surface against a dark background; it may possess one *Schwerpunkt*, *e.g.* painting with person in it; it may possess two *Schwerpunkte*, *e.g.* apparent movement situation with interval for optimal movement; it may possess many *Schwerpunkte*, *e.g.* a landscape painting with no outstanding object included.

c. Patterns may differ in the figure-ground relationship,

i.e., the way in which the figure or homogeneous differentiated portion influences the ground or undifferentiated portion, and vice versa.

5. Patterns may differ according to the number of different structures which may be aroused by looking at them from different perspectives: *e.g.* reversible perspective figures, geometric illusions.

6. Certain patterns may be differentiated according to the type of autonomic response induced. Thus to the observer

a. Patterns may be immediately apprehended as pleasant, unpleasant or painful.

b. Patterns may be apprehended as dangerous or unfriendly or as harmless or friendly.

c. Patterns may be apprehended as interesting or as uninteresting.

The gist of the whole matter is that physical patterns do not furnish us with discrete experiences which can be reproduced merely by keeping the physical conditions constant. Conscious behavior is organized behavior. Consequently most of the complex patterns which appear in conscious behavior have been reconstructed from preexisting patterns in the nervous system. There are some physical patterns which from the standpoint of naive introspective observation hang together and resist structural analysis, and there are other physical patterns which are represented at once fully differentiated in conscious behavior. Moreover, we have no *a priori* rules to specify which physical patterns resist analysis and are represented habitually as undifferentiated in conscious behavior, and which ones fall apart immediately in conscious behavior. *The appropriateness of a physical event for structural analysis by introspective report is a problem that must be solved by experimentation, but nevertheless we do have enough data at hand to convince us that the organism is not capable of rendering introspective reports equally well upon all types of stimuli, nor upon all aspects of all types of stimuli.*[1]

[1] For example the human organism cannot make a static differentiated report upon the nature of the happenings in the path of a rapidly moving object. In order for an observer to get the details of the vehicle of movement the object must persist in the same place or else move at a slow velocity. A temporal and spatial persistence

III. Sense Organs as Conditioners

The efficiency of the functioning of the sense organs varies of course with different observers and with the same observer at different times. Moreover, the sensory mechanisms of observers condition not only the acuity or fineness of differentiation of sensory stimuli, but also the rapidity of response, fatigue, adaptive effects, simultaneous and successive induction in the same sense organ and upon the bilaterally symmetrical sense organ. Numerous interrelations exist between the functioning of different sense organs which have yet to be worked out. The data gathered upon vision far surpass the information upon any other special sense organ, but even in this field a certain amount of uncertainty exists when we attempt to define what is normal for an individual. *We have still a long way to go in our study of the special senses before we can talk about normal human adult sensory equipment and compare one observer with another in this respect.*

IV. Physiological Schemata as Conditioners

The flow of conscious behavior seems to be represented neurologically by a continuous organizing and reorganizing of the potentialities of the manifold physiological patterns. Moreover, a great deal of this reorganization may go on outside of the sphere of analyzed or differentiated experience. Henry Head has noted this fact in studying patients with lesions of the cortex.[2] He observed that the hallucinations so common to this type of disturbance seemed to be due to an undue persistence of the conscious behavior patterns. Normally when cortical activity is perfect we have a continuous change of the locus of nervous activity along different cortical patterns or schemata. The shifting of conscious behavior is presumably paralleled by a shifting of the locus of disturbance among different physiological schemata. Should this oscillatory process be disturbed during waking life, hallucinations occur.

is necessary for a static differentiation, although a dynamic differentiation of say two different types of movement is possible with rapid velocities. See H. R. DeSilva, *Brit. J. Psychol.*, 1929, **19**, 287–291.

[2] H. Head, Aphasia and kindred disorders of speech, Cambridge Univ. Press, 1926.

There are also other organic patterns which exert an important effect upon conscious behavior, namely those schemata which subserve posture, movement, uprightness in vision, and the visceral schemata which furnish, to put it analytically, the sensory core of our affective experience. These organic schemata function more or less continuously, or at least are in readiness to function immediately at any moment. They are extremely important conditioners of the unanalyzed background of conscious behavior, although because of their resistence as well as their rapid and automatic operation we tend to ignore them.

The activity of conscious behavior from the introspective standpoint appears orderly and continuous chiefly because we do not notice the shifts from one locus to another. Sometimes this shift is gradual and sometimes it is abrupt, but even when it is abrupt we seldom note any discontinuity. The nervous system functions continuously, although conscious behavior represents only certain integrated aspects of the total organic functioning. Such terms as consciousness of relation, unconscious contents, *Bewusstseinslagen*, etc., are representative results of the heroic attempt on the part of conscientious observers to describe these abrupt shifts which are the conscious behavior correlates of the shifts of the locus of physiological disturbance from one schema to another.

A novel organization of conscious behavior, such as seems to be necessary to make what we call a choice, requires a brief interval of delay in order to allow for the selection of the most appropriate of the two or more available schemata. These preparatory intervals of physiological organization seem to represent the most common type of undifferentiated conscious behavior which resists descriptive analysis. The path of progress in this direction obviously lies in studying all the conditions which arouse and accompany such undifferentiated cross-sections of conscious behavior, rather than trying further to secure more detailed descriptions of processes which possess little or no detail.

No physical stimulus is significant until it succeeds in developing some stress in the organic integration of emotive

patterns in the central nervous system. Until it releases, resists, depresses or enhances some appropriate visceral schema or emotive pattern it remains one of the physical events that do not matter, *i.e.*, one which exerts little influence upon the direction of conscious behavior. The presence of emotion makes conscious behavior more representative of the whole organism and binds conscious events together more effectively. Studies of learning behavior lead to the conclusion that the particular form of organic activity that we call emotion has the effect of integrating the conscious behavior events accompanying it into better knit units than is the case where there is little or no emotive accompaniment.

An interest seems to be a form of nervous organization which, providing other things are equal, tends to render the orientation of the organism toward that particular form of activity more complete and effective as well as more continuous. An interest is a long-section dynamic mental pattern which tends to spread so as to include the whole organism. Although in following an interest one's conscious behavior goes off on tangents, it presumably continually reactivates some part of the interest schema which is the recurrent locus of the continued activity over a period of time.

V. Individual Differences in Conscious Behavior Patterns

Experimental control of an observer's report by instructions implies more or less passivity, or at least a state of equilibrium on the part of the observer and a coming of the datum or given. Actually the unpracticed observer is hardly ever passive, uniformly attentive, or in a state of equilibrium. The observer untrained in laboratory work is likely to be overexpectant or else bored by fatigue from the mental strain of endeavoring to interrupt and reverse the conscious behavior continuum. The well-trained observer is receptive, but actively receptive. He should be able to remain relatively passive in order to allow a complex mental experience to arise and run its course. Subsequently, he should become exceedingly active in attempting to reconstitute it. Activity and

curiosity during retrospection are just as necessary as unrestricted receptivity during the period when the stimulus is operating.

In most psychological investigations the observer's technique of observation is not independent of the object observed and therefore we must necessarily be more concerned with individual differences. Practically all of our experiences are meaningful and the particular meaning elicited from an observer depends upon the biography of the particular individual concerned. It is useful to refine and standardize our introspective technique by formulating more rules, but what is most needed is more preliminary experimentation and a larger number of observers trained in the use of complete introspection.

We need to reconsider the question of what constitutes a normal useful observer. First of all, of course, the individuals sensory equipment should be studied for variations from the normal. Besides this factor, however, certain habits of conscious behavior which are ordinarily disregarded should be examined. For example, there are several types of obsessions which may seriously effect an individual's observations. Among them are the constancy obsession, the subjective-minded obsession, and the thoroughness obsession.

Some observers are so beset with the notion that there is always a perfect correlation between the external object of common sense and their perception that they interpret their experience in terms of their everyday knowledge of the external situation and consistently render a false report. The properly trained observer must accept whatever stimulus is presented, but not infer it or any part of it in case he has not sensed it. He should appreciate the fact that there is frequently a lack of perfect correlation between the stimulus and perception and be critical of his observations and report.

Some observers are so subjective-minded that they cannot allow a mental process to run its course without interference. They refer everything to themselves to such an extent that they never look back at their reaction with the calm impersonal attitude characteristic of the scientist. Such observers

yield knowledge about emotional complexes and theoretical biases which is interesting genetically, but which has little or no bearing on the problem of the impersonal reactions of a normal human adult to the situation at hand.

The most useful observer is that one who observes details in full and at the same time does not lose sight of the naive whole relationships so common in everyday life. He should aim at thoroughness, but when the details are not present in conscious behavior he should acknowledge his inability to achieve thoroughness. The tendency of an observer to be extremely analytic or synthetic is as likely caused by a mental obsession as it is by an ingrained form of observation, but of course we can never tell about this matter until we have performed some preliminary experimentation with the observer. Often the experimenter can detect systematic obsessions on the part of observers and he should of course attempt to weed them out during the exploratory period.

VI. Laboratory Sophistication

In psychology it is often asserted that introspective reports are rendered inaccurate or at least biassed by the type of psychology which an observer has learned. It is not, however, sophistication as such that leads to inaccuracies. It is slavish sophistication that results in harm. Many scientists in other fields seem to have developed a sort of dual personality in respect to their investigations, that is to say, they keep their impersonal observational self and their creative, theoretical synthetic self quite separate. When called upon, these two selves operate more or less independently, and in this way the scientist can collect and record all sorts of data regardless of whether they uphold his pet theory. There is no reason why the observer in psychology should not also learn to forget theory when he is observing. A good observer in psychology, providing he is intellectually curious and intellectually honest, should be a good observer no matter whether he is observing in an experiment at Cornell University, at Ohio State University or at the University of Berlin. Just as long as observers in such different institutions do their best to

adopt a receptive orientation to the stimulus in order to lose as little as possible of the continuum of events taking place, and describe their experiences as fully and as completely and objectively as possible, and use their technical vocabulary as an aid and not as a fetich, their protocols should be comparable. It should not matter greatly whether an observer uses verbs and describes a vague bit of phenomenon between two bars of light as a 'moving object', or whether he adjectives the movement into 'shimmery greyness' so long as he does not allow the verbal form of his report to stereotype his observational ability. Nor should it matter greatly whether he uses different labels such as aspect, attribute, dimension, characteristic, element, component, feature, member, constituent, part or ingredient to stand for a unit of his experience, so long as the observer does not become obsessed with the idea that such verbal symbols permanently typify conscious behavior units which must always be related or unrelated to their wholes in particular ways under specific conditions. It is the tendency to universalize psychological terms uncritically and make them stand for common and invariable mental processes that renders many psychologists unfit for the task of observation.

Another source of error from sophistication may be traced to the loss of curiosity on the part of the observer. If a psychologist becomes so entranced with the perfection of an extant theory that he loses his sense of wonder at the enigmatic aspects of the flow of conscious behavior, his value as an observer decreases. An appreciation of the defectiveness and limitations of our present descriptions and explanations of mental processes is one of the most necessary traits of a good psychological observer. A belief in the value of psychological observation leads an observer to retrospect more frequently upon his own mental processes and thereby makes him more familiar with them. There is no more important determinant of good psychological observations than an appreciation of the value of them, together with a knowledge of the difficulties and errors involved in making such observations.

In spite of the possibility of error on the part of observers from biassed attitudes and vocabulary fetiches, the greatest

source of error from 'laboratory atmosphere' probably comes from the experimenter. It often arises from an insufficient amount of qualitative exploratory investigation, the lack of which results in too early codification of the instructions. Relevant descriptive data are not called for by the narrow instructions, and even if uncalled for experiences are reported they are often regarded as irrelevant by the experimenter. Most experimenters desire clearcut results and err unwittingly as well as wittingly in interpreting their protocols too ungenerously. Full reports of protocols are rarely printed, and so the unwitting error is bound to occur to some extent because the published article merely represents the experimenter's sample selection of data. If raw scientific data were made available by the investigator even if not published, and more careful reading was accorded those protocols published, this source of error would be eliminated more frequently.

We need in psychology more emphasis upon complete reports[3] rather than upon reports in terms of any one category of mental elements. Far too frequently the observer reports his experiences in visual terms no matter what the sense mode involved. The reason for this obviously lies in the fact that our language is much more richly supplied with words to describe visual experiences than with words to describe other experiences, especially kinesthesis. But if observers realized this common verbal shortcoming they might strive more energetically to overcome it.

We also need more emphasis upon the use of trained observers,[4] adequate preliminary observation and a close cooperation between experimenter and observer. A trained observer can with a minimum of delay give reports upon all of those aspects of the experience which are elicited by the

[3] Complete introspection is defended by R. H. Wheeler, PSYCHOL. REV., 1925, 32, 443–456.

[4] Especially in view of *Gestalt* psychologists' emphasis upon the use of untrained observers. Untrained naive observers are useful for working with a class of undifferentiated and synthetic types of perceptions. But psychologists have also an interest in differentiating between different aspects of a perceptual pattern, and trained observers are better able to carry out this type of analysis. In emphasizing the importance of undifferentiated perceptions *Gestalt* psychologists have apparently gone needlessly to the opposite extreme and denied the usefulness of complete introspection.

specific situation. However, providing a preliminary training period of sufficient length is allowed, nearly any mentally agile and conscientious observer with an average vocabulary can improve upon the accuracy and completeness of his report.

VII. Partial Knowledge and Confrontation

One of the outcomes of our dependence on untrained observers is the custom of keeping subjects as ignorant as possible of the nature and procedure of the experiment. The advisability of imparting full information to all observers is an open question, although we often assume uncritically that it is practically always disadvantageous to let the observer know anything about the experiment. We need less blind acceptance upon this matter and more experimentation. It is quite probable that, if we could make use of trained observers in our experiments, full knowledge might be allowed much more frequently than is the practice at present. The chief reason why an experimenter refrains from imparting information to an observer about his experiment, is that he fears that his observer may be led to falsify his report in an effort either to agree or to disagree with his general findings. If the observer were fully trained and qualified, and possessed a split observer-theorist personality, full knowledge ought to have no effect upon his protocols. However, according to present practice, observers often gain a partial knowledge of the nature of the experiment which may do more harm than full knowledge. A wrong or partially wrong expectation may bring about more discrepancies than a correct expectation. If complete introspection was the ideal and was practiced by every observer, then we could all be propagandists for different psychological systems with impunity. The systematist part of one's conscious behavior can and should be kept separate from the introspectionist part.

Even if the experimenter decides not to give his observers any knowledge at the start concerning the experiment, he should nevertheless consider whether it is advisable to give his observers any knowledge during the course of the experiment. If this possibility does not appeal to him he may find

it desirable to confront his observers with their results at the close of the investigation and ask them for criticisms and additional remarks, or for corroboration or denial of the conclusions drawn. When practicable such a procedure should heighten the accuracy and relevancy of the theoretical conclusions.

The whole mass of data contained in the protocols of all observers should theoretically at least be subject to different interpretations. The experimenter often emphasizes only those data which fulfill certain so-called 'findings.' A nearer approximation to more perfect statistical tools might be obtained if one of the observers or even an outsider went over the protocols and confirmed the interpretations of the experimenter or else reinterpreted them. When an experiment is carefully performed, reinterpretation of old data might prove just as valuable and more economical ofttimes than a repetition of an experiment.

VIII. Analysis

Because of the large amount of impressively accurate work carried out by psychologists adhering to the structural school, the opinion that introspection is an adequate method for investigating clear or differentiated conscious behavior *only*, has come to be generally accepted. An important reason for the rejection of this view lies in the fact that we can never be sure of what is focal or clearest in conscious behavior. There is no line of demarcation between clear and vague experience, but rather the passage from differentiated to undifferentiated experience seems to be of the nature of a gradient.

The final word has not yet been said upon levels of clearness (or differentiation) and probably the final word will be to the effect that the clearness aspect of consciousness has a spatial unity such as focus-margin or figure-ground, a temporal unity such as the specious present, and that its scope varies considerably with the different sense modes. Investigations upon figure-ground situations have shown that in many types of stimuli there are no sharply defined limits between figural and marginal aspects. Considered spatially, conscious be-

havior seems to partake of the nature of a gradient with no line or lines of demarcation between the differentiated and undifferentiated portions. There are likewise no discernible boundaries between the conscious behavior pattern operating at one second and the pattern operating the next second. Transitional states in conscious behavior are frequently undifferentiated, and although difficult to describe we are not justified in neglecting them just because at that moment we could not describe them satisfactorily. For example, processes which were vague or absent in the original experience may be differentiated (or come to a focus) in conscious behavior when we retrospect. Although retrospection may suffer from the addition of details of this sort that did not occupy the same state of differentiation in the original experience, nevertheless such data are of considerable importance because they throw more light on the total pattern in the original conscious behavior, particularly upon those parts which were in a preparatory state of differentiation, and were ready to appear next in conscious behavior.

Most of the investigations upon clearness have been made upon vision, and perhaps clearness can be distinguished only in vision. However, in vision at least there is a gradient of clearness which makes it impossible to say definitely just where clear vision stops and where vague vision begins. The central area of clear or differentiated vision cannot be plotted accurately. The scope of differentiated conscious behavior varies with one's familiarity with the object, analytic and synthetic attitudes and emotional states, such as embarrassment when the peripheral fields become decidedly more blurred than usual. Quantitative error regarding the details of a pattern of conscious behavior may be due either to a careless or underestimated observation of what is differentiated, or else to an overestimated inference of what is differentiated.

Consequently, as there is no definite line of demarcation between what is accurately reportable and what is not, it is unreasonable to assign a zero value to the processes outside of differentiated or clearest conscious behavior. We should give less heed to this hypothetical concept of clearness and

more to the practical task of complete introspection. Observers should be encouraged to report upon everything in the stream of conscious behavior, even if it strikes them at the moment as unimportant or without detail. If an observer limits his report only to clear or differentiated portions of conscious behavior he is likely to abbreviate it unduly.

Even though his material is supposed to be given him directly, a beginner's observations in psychology are as useless as a beginner's observations in histology. An expert observer in a particular subject is nearly always able to discriminate better than an untrained person, because he realizes the need for complete absorption in his task and thus loses fewer of the details of the phenomenon presented. We may state this fact of losing less experience positively by saying that the observer analyzes the situation into elements or details. Structural psychology has given us a certain classificatory schema into which to fit all of our experiences. *Gestalt* psychologists have inveighed against the analysis of the structural school and have emphasized the necessity of noticing neglected relational aspects of perceptual patterns such as figure-ground, objectivity, size, form, speed, localization, brightness, etc. These attributes of experience together with their variable functional interrelations are important, but no new or different species of introspective technique, or the use of untrained observers, are necessary in order to observe them. Observers who are trained to introspect completely can, if they wish, keep their theoretical and observational selves separated and produce protocols acceptable to *Gestalt* psychologists as well as to structural psychologists.

Both gross synthetic units such as descriptions of meaning, as well as common analytic elements such as quality, intensity, etc., are vehicles of conscious behavior and valuable as descriptive units in introspective investigations. *No mental event is ever really simple—it is associated with what has just gone before, with what is going on just outside the differentiated portion of conscious behavior at the moment, and with what is coming next—and therefore whatever descriptive units we care to use in observation will probably vary from time to time.* Just as soon as we

experience an organization in time or space we find relationships and discover that whatever whole we decide to consider is greater than the sum of its parts as indicated by descriptive analysis. There is no unorganized conscious behavior in an awake individual, and therefore there are no unintegrated wholes in the strictest sense. Structural analysis at best is a purely descriptive procedure and we seldom do more in introspection than give an approximate indication of a differentiated cross section of the differentiated as well as undifferentiated conscious behavior continuum.

If we conceive of conscious behavior organismically as a function of a multiplicity of variables operating continuously, we will appreciate the uselessness of bothering ourselves about the question of the simplicity of the unit variable studied. Nothing in conscious behavior is ever simple, because the evidence of biological sciences forces upon us the conclusion that numerous unnoticed conditioning activities are taking place simultaneously in the human organism. In conscious behavior we always find elements related in a spatial-temporal pattern. To analyze a bit of conscious behavior and describe it in terms of temporally static and spatially fixed elements and omit the 'and—relationships' and 'organic relations between parts of the conscious process' is to fall short of adequate description. Recognition of the fact that the conscious behavior continuum is representative of only a part of the total functioning of the organism is a view which needs careful consideration by introspective psychology.[5] Individual retrospective reports therefore are representative of only a part of the events which underlie the organization of conscious behavior. Perhaps if we admit that undifferentiated sections of conscious behavior are really and truly undifferentiated and turn our attention to a study of the types of total situations which evoke undifferentiated experiences rather than spend our time inventing names such as 'unconscious contents' to describe undifferentiated or empty sections of conscious behavior, we might help to pull qualitative introspective psychology out of the rut into which it has fallen.

[5] See R. H. Wheeler's, 'The Science of Psychology,' Crowell, 1929, for a textbook treatment of this viewpoint.

IX. Summary

The common sense of introspection, like the common sense of any other field, involves an appreciation of points of view that we have overlooked or forgotten in our first enthusiasms for original but unnecessarily isolated techniques, namely:

1. That we can keep our theory distinct from our observation, if we want to, by cultivating split observer-theorist personalities. We need not therefore resort to naive observers in order to obtain 'natural' unbiassed reports. *Gestalt* psychologists have rendered an inestimable service by attempting to classify mental experiences upon the basis of physical patterns, but they have gone to the extreme by employing untrained observers and by limiting their study mainly to synthetic experiences. A normal, useful observer whether naive or trained is one who is free from certain obsessions which are detrimental to psychological observation.

2. That an incoming stimulus cannot be represented in a constant and uniformly differentiated fashion in conscious behavior, because on each occasion it must reconstitute a correlative neural schema which is never exactly the same. Moreover, even while this neural schema is being activated it may be altered in the process of being blended into the conscious behavior continuum which possesses an ever changing phenomenal spatial orientation and psychological connectedness.

3. That the conscious behavior continuum is representative of only a part of the entire activity of the organism, and is therefore conditioned by unnoticed changes in other parts of the organism, as well as by unnoticed changes in the outer environment which may not be clearly differentiated in conscious behavior but which nevertheless may be significant.

[MS. received July 10, 1929]

DISCUSSION

THE LADD-FRANKLIN BLUES

In explanation of Plate VII in her book, 'Colour and Colour Theories,'[1] Dr. Ladd-Franklin writes the following: "The diffraction spectrum (not distorted by the excessive dispersion which the prism spectrum effects at the high-frequency end) and its after-image (residual image). No one who has once seen this beautiful phenomenon will ever again believe (Hering) that red and green are complementary colours. *Red, green, and blue* are a *complement-triad* (the great discovery of Thomas Young . . .), and the residual image after red light has struck the retina is blue-green; after green light has struck the retina it is blue-red (purple), as the picture shows; but the complement to blue is yellow, the complement to yellow is blue."

Dr. Ladd-Franklin's 'picture' is indeed descriptive of the things she says; but "no one who has once seen this remarkable phenomenon" of the diffraction spectrum will ever recognize any resemblance to her Plate!

The Ladd-Franklin theory of color vision, clever as it is in its 'higher synthesis' of physiological, physical and psychological matters, nevertheless falls short when a bit of simple observation is applied to it. Let me innumerate a few points, particularly those discrepancies that become obvious on consideration of the Ladd-Franklin blue, and blues in general.

1. Her development plan from white to yellow and blue, then from yellow to red and green, allows for a convenient graph of physical and psychological facts. She compliments the work of Young and Helmholtz by pointing out that mixtures of red and green (as in physics) revert to yellow, and that mixtures of yellow and blue revert to white. She compliments much of Hering's persistent claims for yellow by giving it a prominent place in her theory. All this is smooth and orderly until her blue is brought to the witness stand!

2. The blue of the Ladd-Franklin theory is a good visual unitary blue. *It is not, however, the unitary blue of the physicist,* which is not a true blue at all, but a decided violet-blue! In other words, the

[1] New York, Harcourt, Brace, 1929, pp. 277.

Ladd-Franklin blue shown in Plate VII of her book will not, in physical mixture with yellow, revert to white. Rather, such a blue mixed with yellow will form a pale green. This throws her theory entirely out of line and reveals that it does not so definitely ally physical fact with physiological and psychological fact.

3. In her use of this blue (which is, no doubt, a good *visual* unitary color) she is forced to neglect mention of the fact that the *physical* complement of blue is red, not yellow—that physical mixtures of red and blue form white. She has already put her blue to use with yellow and now must choose blue-green as the complement of red and here depart from a loyalty to the physicist which she has formerly held.

4. If, on the other hand, the 'blue' of her theory is to be considered the same blue as the physicist's—as she plainly wishes to infer—her entire development plan is compromised. First of all, the physicist's blue (blue-violet) is not the residual after-image of yellow. Thus there is no true visual complementation with the two, and there is no reason to assume that such a blue (blue-violet) plus yellow will in vision revert to white. Secondly, the physicist's blue (blue-violet) in physical mixture with green forms a blue which, in physics, becomes a color-blend. This color-blend blue by a big and convincing margin more nearly approaches the blue of Ladd-Franklin than the true blue (blue-violet) of the physicist which she makes out is being supported. Thirdly, if Dr. Ladd-Franklin were to change her blue to the physicist's blue (blue-violet) she would then destroy its unitary significance from the psychological standpoint, for no conceivable argument could be presented to prove that the physicist's blue (blue-violet) is at all unitary in vision. (It can be formed in a minute with red and blue disks on the color wheel.)

Apparently the big difficulty encountered in theories of color vision lies in the inability to establish universally acceptable primaries. The physicist with his primaries has an easy task, for one look at the spectrum easily convinces one that red, green and blue-violet are unitary colors combining to form yellow, blue and magenta. This fact becomes outstandingly evident with even a small amount of experimentation with light rays. Visual primaries, however are not so conveniently fixed. Hering's crimson and verdigris while they are hues that show much evidence of being visual primaries (they are two stable and invariable colors that do not undergo a change in hue as their stimulus passes from the center to the periphery of the retina), are not accepted as primary by Ladd-Frank-

lin, and her reasons for not accepting them are altogether logical. In fact, much of the dispute between the Hering and the Ladd-Franklin theories rests with the question, are crimson and verdigris or are the physicist's pure red and green the exact visual primaries?

On the selection of a blue, however, both Hering and Ladd-Franklin are agreed, and both use the same blue. Because Hering pays no attention to the physicist, his blue conforms entirely with the claims he makes for it. But the same blue, claimed by Dr. Ladd-Franklin to be both a physical and a visual primary, becomes rather grotesque in view of the things she has to say for it and the persistent way in which she tries to ally it with yellow both for physical and visual complementation. In truth, her blue has no more justice from the physicist's standpoint than Hering's crimson and verdigris which meet with constant objection from her.

How, then, is Dr. Ladd-Franklin to settle with her blue? Is she to adhere to her particular chroma shown in Plate VII of her book, make yellow out of red and green to the physicist's satisfaction, and then try to make white out of yellow and her untrue physical blue? Or is she to change her blue to a true physical blue (blue-violet) and thereby jeopardize her whole development plan by trying to ally such a blue with yellow and make the two of them show an impossible visual complementation?

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[MS. received August 18, 1929]

GUNDLACH'S FOUR SOURCES OF CONFUSION IN PSYCHOLOGICAL THEORIZING[1]

Professor Gundlach is quite correct in supposing that in my postulates I have tried to follow the procedure of mathematics or at least that of the natural sciences. However, since psychology has not yet developed the degree of uniformity in its fundamental principles that is found in mathematics, it is not surprising that my postulates do not meet the mathematical criteria. It is true that I use the physical electronic hypothesis as the basis of the psychological postulates. I do this in order to indicate (1) the direction in which I believe the analysis of human behavior is going, (2) to show the progression of steps from electronic to compound multicellular or social organization. I point out that the validity of these assumptions is a physical problem. In time we may find these assumptions unnecessary, but at present it seems to me that it is helpful to indicate the type of premise upon which one's psychological principles are based.

I do not believe my critic should expect any set of psychological postulates which can be formulated during the present controversial stage of psychology to be much more than an attempt to state clearly some of the basic assumptions underlying a given system. Perhaps my postulates do not deserve the name postulate; I admit this.

I am in hearty sympathy with Gundlach's attempt to apply Broad's classification to psychology. However, as I see it, a verification of these twenty mind-body views can only come from a study of sophisticated adult behavior. Certainly the data from genetic and social psychology seem to find no place in Gundlach's list. Nor do we find any explicit statement as to the nature of language. Of course it may be said that these topics should be excluded from psychology *as such*. Yet the fact remains that a considerable proportion of the professional psychologists are investigating such problems; a far greater number, it seems to me, than those who are investigating the differentia between the primary, secondary, and emergent qualities which form the core of Gundlach's problems.

I may be wrong in my interpretation, and if so, I hope this will

[1] Psychol. Rev., 1929, 36, 285–306.

stimulate Professor Gundlach to elaborate his principles for the specific use of psychologists. However, I do not admit that my postulates are incomplete because they do not specifically include 'analytic facts of experience,' or 'facts of configuration.' Of course, it is easy for me to ask what are these analytic facts of experience, or configuration, which are excluded. I believe I am well within the scope of scientific method when I expect my critic to make this analysis. I only stress human behavior.

When someone has formulated 'a theoretical basis of experience or configuration' it will be possible to make the comparison and determine whether a study of human behavior from the genetic and the biosocial standpoint will not conform with what psychologists are actually doing or expect to do. That my postulates do not meet the epistemological requirements of all philosophical systems does not disturb me at all. I am only concerned with their scientific utility.

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